Comment on Stuart Shanker, What Children Know When They Know What a Name Is: The Non-Cartesian View of Language

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What Children Know When They Know What a Name Is

The Non-Cartesian View of Language Acquisition[1]

by Stuart Shanker

Nativist theories of language insist that an infant must possess some abstract concepts about the structure of language or, at the very least, some word-learning biases to be able to acquire the sorts of skills and knowledge displayed by competent language-speakers. A direct consequence of Cartesian epistemology, nativism limits the role of linguistic anthropology to validating its claim that children typically acquire language in essentially the same manner, regardless of the culture in which they are raised. It seeks to confine linguistic anthropology to the study of the socialization processes whereby children use their "innate" linguistic knowledge to become accepted members of their community. Linguistic anthropologists, in contrast, see field studies as a way of discovering what children in different societies actually learn about a language when they learn how to speak. In this non-Cartesian approach, children are seen as learning how to do different kinds of things with words—how to engage in the culturally significant actions that make up their community's "form of life." The case of proper names in Anglo-American and Navaho culture is here examined as an illustration of the significance of this epistemological shift.

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For a philosopher of language first exposed to linguistic anthropology through the writings of nativist theorists, it is something of a shock to read the actual anthropological texts, for the approach to the study of language development carved out by linguistic anthropologists bears scant resemblance to the manner in which it has been represented in nativist writings (see Pinker 1994, Crago et al. 1997, Gopnik et al. 1997). Far from constituting the ethnographic contingent in the nativist search for universal structures, linguistic anthropologists have opened up a very different way of looking at language development both in itself and in terms of its relation to other aspects of child development. What this aspect shift calls into question is not just the nativist view of language acquisition as a maturational process but, more fundamentally, the Cartesian assumptions that underpin that view.

A philosopher of language can, of course, be expected to be preoccupied with epistemological questions, but linguistic anthropologists have been just as concerned with the problems raised by Cartesian epistemology. Nativism—which, as we shall see, is the direct consequence of Cartesian epistemology—establishes a paradigm that dictates the role that anthropology can play in the study of a child's linguistic development. In essence, the nativist insists that an infant must possess some abstract concepts about the structure of language or, at the very least, some word-learning biases to be able to acquire the sorts of skills and knowledge displayed by competent language-speakers. In this respect, nativist thinking about language appropriates for itself the title, once claimed by Enlightenment philosophers, of "Queen of the Sciences" while anthropology is relegated to the status of a footman. The question that linguistic anthropology forces us to address, however, is how our views about the nature and development of language must change once our thinking is freed from the dictates of Cartesian epistemology.

The most important contemporary nativist theorist about language is, of course, Noam Chomsky. Chomsky (1959) claimed in his famous "poverty of the stimulus" argument that children must know general principles of syntax that could not possibly have been acquired from input but must be built into the human mind. More recently, constraint theorists have argued that children must come to the task of acquiring category terms equipped with "some assumptions about the nature of categories and about the nature of category terms" that limit the kinds of hypotheses they consider (Markman 1989:7). Thus the Chomskyan nativist concludes that children must know such things as that the structure of a sentence is based on the relationship between phrases rather than on the linear sequence of words, while the constraint-theory nativist maintains that they must be biased to interpret each new word as having an extension

1. This paper was written in close cooperation with Willow Powers, an adjunct professor of anthropology at the University of New Mexico. In addition to discussing every point contained in this paper, she was my primary resource for the discussion of Navajo naming practices, and she played an instrumental role in developing the anthropological view of the meaning of proper names presented in the final section. I am also grateful to Keith Basso, Penny Brown, Jeff Coulter, Barbara King, and Peter Hacker for their considerable help and to the anonymous readers of the first draft of this paper, who made many invaluable comments. Finally, my enormous debt to Talbot Taylor is manifest throughout this paper, particularly in the discussion of the reflexivity of proper names.

that excludes that of other words. Such "structural principles" or "learning biases" are said to be innate in the uniquely genetic-determinist sense that they are biological features of the manner in which the human mind/brain automatically processes language (spoken or signed).

These arguments have been subjected to extensive philosophical critiques of the nativist's use of epistemic operators (Baker and Hacker 1984*b*, Shanker 1996). In essence the argument here is that by describing a mind/brain as "knowing," for example, that the structure of a sentence is based on the relationship between phrases one violates the conditions that govern the use of "knowing" or "believing" in that these operators can be significantly applied only to *agents*. But the nativist responds that, even if it makes no sense to speak of a mind/brain as "knowing" the principle of structure-dependency, one need simply invent a new term and speak, for example, of the mind/brain as "cognizing" this principle (Chomsky 1980). As far as the nativist is concerned, one judges a theory not by how well it accords with the way we *ordinarily talk* about the phenomenon in question but by what it explains and predicts. Hence all that matters is whether children typically acquire language in essentially the same manner, regardless of the culture in which they are raised, and whether it is necessary to postulate nativist principles in order to explain this.

The role that the nativist assigns to linguistic anthropology is to validate these claims. Thus Chomskyan nativists—or, as they are more commonly known, "generativists"—have been drawn to cross-linguistic studies as a way of buttressing their view of the invariable versus the socially constructed properties of language and, more precisely, as a way of refuting the empiricist view that social factors are essential for linguistic development. In particular, generativists have sought to show that "motherese"[2] is not a cultural universal—that different cultures assign different agents (e.g., older siblings) to act as primary caregivers for considerable periods of time and that different societies display widely differing amounts of motherese and in some cases none at all (Pinker 1994).

Generativists thus seek to establish that, given that there are "universal properties of language" but no "universal formats of interaction," it follows that the former *must* be due to innate factors—to *universal properties of the human mind* (Crago et al. 1997). Crago and colleagues concede, however, that, so far, the only "universals" revealed by cross-linguistic research are (1) "the presence of language used interactively but not necessarily in interaction with the child" and (2) "the presence of an affectionate interactional relationship between the child and others" (p. 76). These two principles do not in themselves constitute a defence of nativist epistemology. On the contrary, they are precisely the themes stressed by interactionists, who are fundamentally opposed to the nativist outlook that one finds in generativism and constraint theory. What separates nativists and interactionists is not, however, simply that the latter place much more emphasis on the importance of social and communicational factors for language development (see Shanker n.d.*a*). More fundamentally, interactionists insist that "language is learned not because it is a private symbol system, but because it is a means of communicating with others. Language is embedded in a social context, from the earliest rudiments of language learning to subsequent adult use" (Goldstein and Hockenberger 1991:402). In other words, interactionists repudiate the Cartesian epistemological assumptions that underpin both generativism and constraint theory.

Clearly, then, generativists and interactionists must have significantly different interpretations of each of the above two principles. The generativist reading of the first point is that, for some reason (undoubtedly genetic), the child's mind/brain seems to respond or be attuned only to language that is used interactively and, hence, that just being exposed to an artificial language source (e.g., a television or a radio) is not sufficient for a child to acquire language but that one should not confuse the factors necessary to potentiate the information contained within the "language gene" with the actual content of that information (which is genetically encoded). As far as concerns the second point, the generativist regards a child's emotional development as an important *extrinsic* factor for language acquisition in that it may accelerate or impede the maturational processes involved but considers emotional development incapable of affecting what a child "innately knows" about language.

The same point applies to cross-linguistic studies and thus accounts for the role that nativism assigns to linguistic anthropology. The nativist is primarily interested in substantiating the claim that "the commonality of the language milestones across cultures and languages leads to the conclusion that there are certain fundamental innate characteristics to the human mind involved in the acquisition of language" (Crago et al. 1997:87). But then, the more linguistic anthropology has advanced, the shorter has become "the list of universal operating principles" (Slobin 1992). Indeed, one might argue that, if linguistic anthropology has buttressed anything, it is the early descriptivist view that children in different societies master different grammatical constructions in different orders at different ages. How can the nativist reconcile this evidence of pronounced cultural variability with the premise that "the linguistic properties of the mind can be seen as fashioning the variable aspects of language into an elegant tapestry of multiplicity and at the same time rendering the common features of acquisition into a statement of equal mental capacity" (Crago et al. 1997:87)?

The standard nativist response to this objection is that "the extent of the variability . . . does not prove that there is nothing at the core" (Crago et al. 1997:87). What is said to be innate is the *capacity* to acquire the grammar of the language used in a particular social environment,

2. "Motherese," or "child-directed speech," refers to the tonally and grammatically distinctive manner in which caregivers typically speak to infants (Bruner 1983).

and the task that confronts language theorists is to explain the innate constraints on which this capacity rests. The fact that a child at such-and-such an age may not yet use some construction merely signifies that the construction in question has not yet matured. The actual sequence of language-specific structures that a child acquires falls under the heading of the "socially constructed" properties of language that are determined by the frequency with which different societies use different constructions.[3] The basic "grammatical principles" themselves "are available to the child at birth and remain constant throughout development. The variation in the timing of acquisition is due to language-specific factors" (Crago et al. 1997:81).

In other words, "it is assumed that an innate capacity for grammatical structure is instantiated in the biological make-up of the individual child, [and] the specific and variable aspects of particular languages . . . influence the exact manner in which this structure gets filled in" (Crago et al. 1997:82). It is important to be clear, however, that the nativist is not interested here in the child's perceptual apparatus, in basic cognitive mechanisms such as memory or attention span, in the constraints on dyadic interaction that have come about as a result of the immaturity and plasticity of the infant's brain, or in the biological traits which dispose a child, under the appropriate developmental circumstances, to become a social agent (Shanker and Taylor 2001). Rather, the nativist believes that "complex abilities like cognition [or language are] 'inside us' all along, albeit in smaller form, and get passed on to subsequent generations in that form, and just, as it were, 'grow' in individuals" (Richardson 1998:2).

According to this nativist perspective, the primary task of linguistic anthropology is simply to confirm that there is nothing in the cross-linguistic data to disprove this view of the child's "epistemic" biological make-up. If the core of the nativist hypothesis is simply that all children possess a "language capacity," then cross-linguistic data must *ipso facto* verify that core. If what children know at birth is the most abstract properties of language, then the fact that they acquire the particular natural languages to which they are exposed *proves* that they possess that "knowledge."

Thus, nativism allows no scope for linguistic anthropology to play a positive role in our understanding of how we know what we know about language (Bowerman and Levinson 2001). Cross-linguistic studies are seen not as an investigation of what the children of different societies learn when they learn how to speak but, rather, as studies of how children learn to *apply* their "innate knowledge" of language. That is, nativism seeks to confine linguistic anthropology to the role of studying the socialization processes whereby children use their "innate" linguistic knowledge to become accepted members of their community, for what children "know" about language when they learn how to speak has been defined a priori. But linguistic anthropologists have adopted the opposite point of view: instead of looking at field studies as a way of validating (or refuting) the precepts of Cartesian epistemology, they see them as a way of *discovering* what children in different societies actually learn about language when they learn how to speak. The implications of this epistemological shift for our understanding of the nature of language and language development is one of the most significant of the many questions that linguistic anthropology raises.

3. An argument, incidentally, that is reminiscent of scaffolding theory.

Non-Cartesian Epistemology

The most obvious problem that one faces when seeking to explain language development in non-nativist terms is that children learn how to speak in such a short time and that so much of their language learning seems to be automatic. The apparent ease with which children acquire language appears especially remarkable when one considers the amount of effort that adults must make when learning a second language. The problem is that, once one starts explaining the acquisition of these skills as the result of children's "tacit knowledge" of the structure of language, one can postulate any number of abtruse rules based on patterned regularities discovered by language theorists. For example, when children say, "I'm hungry," do they know, implicitly, that "I" is a pronoun, "am" is the first-person present tense of the verb "to be," and "hungry" is an adjective? Do they know that "I'm" is a contraction, and do they know the different rules for forming first-person, second-person, and third-person contractions? Are these metalinguistic rules or the even more abstract generalizations postulated by generativist theory even relevant when discussing children's developing linguistic skills? Can one talk about the development of *skills*—any skills—in a completely decontextualized manner?

To answer these questions, let us adopt a much more literal position than that of the nativist: when we ask what it is that children know when they say, "I'm hungry," what we are interested in is what they know and not what their mind/brains might be said to "cognize." And let us be clear that the reason we are interested in this issue is that we want to find out whether non-nativist and nativist answers to the question of what children know when they learn how to speak simply operate at different—but compatible—levels of explanation or whether they actually conceptualize language development in significantly different terms. What we want to investigate here is whether linguistic anthropology, which has sought to free itself from Cartesian epistemological assumptions, leads to an altogether different picture of *what* children are mastering when they master language skills and therefore *how* they acquire language. Furthermore, the point of such a question is not to dispute the possibility that children typically display consistent regularities that amount to milestones in their linguistic development. Rather, it is to question the nativist conception of these regularities—to investigate

whether these milestones prove that the development of language is "under maturational control," that is, neurobiologically determined by "unitary timing constraints" (Petitto 1997:51), or whether their proper elucidation lies at an altogether different level of explanation.

Let us consider some of the epistemic stages through which children (typically) proceed in their linguistic development. The one minimal thing we are inclined to say is that, when children say, "I'm hungry," they know, in fact, that they are hungry. But, of course, this need not be the case, even if they actually are hungry. In Thorndike's (2000) terms, an association between making the sound [aim hungri] and being fed may simply have been "stamped in." This is precisely the sceptic's point when he criticizes ape-language research—that the ape does not know what "I'm hungry" means even though its "trainers" respond to its communicative behaviour as if it did (Pinker 1994). So too with children in this case: they don't even know that making this sound is an effective way to get food, for in this context they don't *know* anything. The sound [aim hungri] is no different here from a newborn's hungry cry.

Further, even when children *intentionally* make the sound [aim hungri] in order to be fed, this does not entail that they know that [aim hungri] *means* something. It may simply be the case that they have learned that making this sound has such-and-such an effect on their caregivers. (Severely autistic children often vocalize in this manner.) Or they may simply be making the sound because they like it or find it soothing. Again, even when they realize that people make sounds to inform each other about certain things, they may say [aim hungri] when they are tired as well as when they are hungry. And, much to their parents' consternation, they may become intensely agitated when they say [aim hungri] and their parents insist on feeding them.

Or it may be the case that, from their perspective, what they are saying is [aimhungri]. Perhaps they know that this informs their parents that they are hungry, but they don't know that their unarticulated expression can be broken down into components, let alone the parts of speech assigned by theoretical linguists. (A similar occurrence is fairly common when learning a foreign language.) Or they may say both [aim hungri] and [hungri aim], possibly but not necessarily with different intentions in mind.

Or perhaps they say [aim hungri] to tell their parents that they are hungry but think that [aim] is one of their pet names, like "sweetheart." Children have a lot of trouble mastering the use of pronouns, and it may be some time after they've been saying "I'm hungry" before they come to master the indexical nature of "I" (Owens 1996). Indeed, one of the more intriguing features of the language of autistic children is that they often reverse their pronouns, saying, for example, "You're hungry" if they want to be fed.

Finally, even once they have mastered the basic use of "I'm hungry" and know when to say "I'm hungry," "you're hungry," and "she's (or he's) hungry," adults still know all sorts of things that are beyond them. For example, adults who have been schooled will likely know that "I" is a pronoun, "am" is the first-person present tense of the verb "to be," and "hungry" is an adjective (that is, they will know how to use "pronoun," "tense," and "adjective"). And, of course, children don't understand why their parents are so annoyed with them when they announce "I'm hungry" during communion or why they can't just walk up to a stranger on the street and say this, even though it is painfully true.

The nativist will respond that the various steps outlined above confuse two different epistemic issues—what a *child learns* and what a *mind/brain* (*innately*) *knows*. The former aspect of linguistic knowledge is said to lie in the realm of socialization, whereas the latter concerns formal knowledge of the most general principles of language. And further, it is this formal knowledge that, according to the nativist, enables the child to learn, for example, what the particular sound [aim hungri] refers to and when it is appropriate to say "I'm hungry." The nativist is interested in *the universal properties of the human mind* that make language use possible and not in the particulars of culture-specific language uses.

The nativist depicts this epistemic distinction as the difference between *language socialization* and *biological endowment* (Pinker 1994). In essence, the nativist is drawing a fundamental epistemological distinction here between what an individual learns and what the species knows. Indeed, it is this distinction that underpins the current debate over the "language gene" hypothesis (Shanker n.d.*a*). According to this hypothesis, individuals are culture-bound creatures—the bearers of customs, intentions, and volitions—whereas mind/brains exist at a supraindividual level that transcends the dynamics of families or communities. Individuals require effort and practice to master the conventions of their societies, but the human mind/brain has been programmed by natural selection to process different kinds of information in different ways. The manner in which individuals learn how to do things with words may vary from society to society, but the manner in which the language component of the mind/brain processes linguistic information is thought to be predetermined (barring brain damage, genetic mutations, etc.) and to operate mechanically.

To clarify the nature of the distinction that the nativist is applying to language development, we might consider the manner in which different societies categorize the primary colours (even if it is the case that individuals tend to identify the same prototypical samples of paradigm primary colours). Human colour vision universally depends on the interaction of three types of cone cells: one especially sensitive to red light, another to green light, and a third to blue light. Each human cone cell absorbs light in only one of these three sectors of the spectrum. It now appears that the receptor proteins in all these cones use retinal, a derivative of vitamin A, to absorb light, and each tunes the retinal to absorb a different range of wavelengths (cf. Fodor 1983).

Nativist theories of language acquisition are proposing

that a similar distinction between the biological and the social applies to language, where, for example, semantics and pragmatics are variable social phenomena and some aspect(s) of morphosyntax are a universal processing phenomenon. But then, the question we are concerned with in this paper is whether the analogy limps badly precisely at the level of nativist epistemology. One would hardly want to succumb to the homunculus fallacy of arguing that the fact that different cone cells are sensitive to different light spectra amounts to the same thing as saying that these different cone cells *know* ("cognize") the difference between red light, green light, and blue light. Rather, the subprocesses involved in colour vision are completely mechanical. It is individuals, not a part of their visual system, who see red, green, and blue—who know that such-and-such a colour is "red," "green," or "blue."

This is not to say that there are no subprocesses that are relevant to language development in a way that is comparable to the subprocesses involved in colour vision. Clearly, for example, a child's ability to develop language skills depends on basic capacities relating to perception (visual and/or auditory), attention, and inhibition.[4] But to say that a child must possess a properly functioning "general auditory processing mechanism" to draw categorical distinctions between phonemes (Kuhl and Meltzoff 1997)—for example, that a child must be able to hear the difference between the sounds /ba/ and /pa/—does not amount to the same thing as saying that the child is born with the "implicit knowledge" that a phoneme is the smallest linguistic unit of sound which can signal a difference in meaning.

Indeed, the more we learn about the processes involved in attentional learning and the richly structured social environment in which children acquire their first words, the less compelling it becomes to posit special-purpose word-learning mechanisms (see Tallal and Stark 1981, Tomasello 2001). Still, mustn't children know at least some very general things about language to be able to learn how to use a proper name correctly? Mustn't they at least know, for example, that a sound can be used as a name for a *person* and that the sounds that we use as names for people have different linguistic properties from the sounds we use to describe what those people are doing? Without some sort of prior knowledge, how could children possibly infer that the speakers in their community were referring to the person and not to one of his actions when they said his name? Clearly, we need to look more closely at this fundamental epistemological problem before we can address the significance of viewing linguistic anthropology as a way of *discovering* what children learn about language when they learn how to speak.

4. We must be careful, however, not to assume that the same biological and psychological processes are present in all children, for the more we study children with developmental disorders, the more we learn about the atypical processes whereby some children arrive at language-matched skills (Shanker n.d.*a*). Moreover, the possibility that the phenomenon of equifinality—the fact that developing organisms of the same species can reach the same endpoint via different developmental pathways—may apply to language development in the normal population as well cannot be ruled out. The norms on which the maturational picture is based are merely descriptive, not explanatory devices based on the typical stages observed in large samples of children. Nor can we rule out the possibility that different language environments can have different influences on children's basic biological and psychological capacities.

Radical Indeterminacy of Translation

In order to appreciate the full significance of cross-linguistic studies, we need first to recognize that nativism is a response to a *philosophical*, not an empirical sceptical problem. All too often one falls into the trap of searching for ways to respond to this sceptical challenge without looking at the nature of the problem itself. No better example of the importance of this point could be found than Quine's famous argument for the radical indeterminacy of translation. According to Quine, a field linguist trying to construct a dictionary for the language of some isolated community could never be certain about having correctly translated the sound "gavagai," which the members of the community utter while observing or pointing at a rabbit. It is now customary to cite this argument as proving that language would be unlearnable without at least some background assumptions.

Significantly, this conclusion is strikingly similar to the argument that Chomsky presented at much the same time. Indeed, the famous Chomsky-Quine debate of the 1970s was possible at all because, much as Quine and Chomsky may have disagreed with each other in their views about the nature of language and how it is acquired, they made the same epistemological assumptions (Shanker 1996). It is the *source* of these assumptions that we need to uncover if we are to free linguistic anthropology from the dictates of nativist epistemology.

Quine clearly intended for his argument to be read in anthropological, not hermeneutic, terms. His radical translator is not supposed to be some solitary scholar struggling over the text of a dead language. Rather, the translator is meant to be an ethnographer, freely interacting with informants. The ethnographer can ask them anything at all about the meaning of "gavagai," go on gavagai hunts with them, learn how to identify gavagai tracks, spot gavagai warrens, prepare gavagai stew, bargain for gavagai pelts, raise gavagai pets, all the while never quite being sure whether "gavagai" means "rabbit" or "rabbit-stage" or "There it goes."

As far as ethnography is concerned, it is important to bear in mind that Quine provides no reason to suppose that something has happened that the field linguist cannot comprehend—some use of "gavagai" that simply lies outside the realm of the ethnographer's experience. Quine has no interest here in ethnographic methodology. Indeed, as he sets up his problem, even if the informants were to judge the ethnographer's use of "gavagai" correct it would have no bearing on the sceptical problem that he has raised.

Part of the fascination of this argument lies in the fact that it is so counterintuitive. Surely, one wants to argue, the manner in which a community thinks must be transparent in the way its members speak and act. Surely observation and participation together will establish whether someone is referring to a rabbit or to one of its parts. But Quine's strategy lies precisely in the fact that this is one's immediate response. In effect, he baits the trap with "gavagai" and then springs it shut with the follow-up point that whatever questions we ask to determine the reference of "gavagai" will depend upon a prior translation of what he calls our "individuation apparatus" (e.g., indefinite and definite articles). But the same sceptical problem arises in regard to this individuation apparatus (Quine 1969:33) as occurred with "gavagai."

Quine's next step is to apply the same argument to count nouns that are used both as concrete general terms and as abstract singular terms (e.g., "green"). The example here of a colour word prepares us for the *coup de grâce*, which is that this sceptical problem applies just as forcefully to speaking one's own language as to cases of translation.[5] In other words, the upshot of the indeterminacy-of-translation argument is that "referential inscrutability" is inherent to language use—so much so that this third-person sceptical problem can even be extended to first-person uses (Quine 1969:47).

Thus, Quine's indeterminacy-of-translation argument is an instantiation of a much larger sceptical problem that is intrinsic to Cartesianism, and much of the argument's effectiveness stems from this epistemological framework that frustrates our every attempt to escape the dilemma in which Quine lands us. The problem here, as Quine presents it, is that the ethnographer can only *infer* from the informants' behaviour what "gavagai" refers to; he "has no access to native meanings apart from what he can glean from the observed circumstances of utterances" (Quine 1970:14). That is, Quine embraces from the start the standard Cartesian assumption that *meaning* and *reference* are mental phenomena, which as such are epistemically private. The reason meaning and reference are inscrutable, therefore, is precisely that everything mental is inscrutable. The indeterminacy of translation is simply part of the larger Cartesian problem of the indeterminacy of the mental.

Quine stresses, however, that, even though ethnographers can never be certain that they know what their informants are thinking when they talk about "gavagai"—even though *I* can never be certain that I know what my neighbour is thinking when she says, "That ⇗ is red," or, for that matter, what I myself am thinking when I call something red—the fact is that we nonetheless successfully interact with one another verbally. Just as the radical translator can map different referents onto "gavagai" with no discernible anomalies in the informants' behaviour, so too we learn how to use "red" correctly even though each of us may experience a different colour sensation when we look at red things. Quine concludes that, as far as the scientific explanation of linguistic intercourse is concerned, *meaning* and *reference* must be idly turning wheels, for "the uniformity that unites us in communication and belief is a uniformity of resultant patterns overlying a chaotic subjective diversity of connections between words and experience" (Quine 1960:8). In other words, "words mean only as their use in sentences is conditioned to sensory stimuli, verbal and otherwise," and in turn, we use language in the expectation that it will cause other agents to do such-and-such (Quine 1960:17, ix, 8).

Quine was mounting a deliberate challenge to the prevailing view that Chomsky's review of Skinner's (1957) *Verbal Behavior* had dealt a fatal blow to behaviourism, and therein lies the key to his intentions in the radical-indeterminacy-of-translation argument. Quine's goal was not to convince us that we never successfully communicate with one another; it was to convince us that we must formulate a different scientific account of the nature of successful communication, one that makes no appeal to any of the "old notions of meaning, idea, proposition" that land us in such sceptical dilemmas as that highlighted by the indeterminacy-of-translation argument (Quine 1969:304). Quine wants us to accept that there is no categorial distinction between language *qua* verbal behaviour and behaviour *simpliciter*, that is, that the contingencies that cause a rat to depress a lever are no different from those that cause a child to say "gavagai" (see Skinner 1957). Hence the scientific explanation of verbal behaviour must be conducted entirely in terms of behavioural contingencies and uniform stimulus conditions.[6]

Quine's account of verbal behaviour is subject to its own set of internal strains and inconsistencies (Shanker 1996), but what interests us here is simply the fact that his defence of behaviourist theory rests on the assumption that "words are out where we can see and hear them," unlike meaning and reference, which are hidden from observation and perhaps from introspection (Quine 1969:35). In this Cartesian picture, the fundamental problem that must be explained in the study of language development is how a child could possibly get from a null cognitive state to knowing how to use as complex a system as language without internal constraints. Thus, much as he may have disliked the nativist premise that there must be universal properties of the human mind that make language acquisition possible, even Quine was forced in the end to concede that "any behaviorist account of the learning process is openly and emphatically committed to innate beginnings. The behaviorist rec-

5. For example, "We can systematically reconstrue our neighbor's apparent references to rabbits as really references to rabbit stages. . . . We can reconcile all this with our neighbor's verbal behavior by cunningly readjusting our translations of his various connecting predicates so as to compensate for the switch of ontology" (Quine 1969:47).

6. For example, a child is conditioned to say "red" in the presence of red stimuli: "'Red' . . . is a happy case where a nearly uniform stimulatory condition is shared by simultaneous observers. All the assembles retinas are irradiated by substantially the same red light" (Quine 1960:7).

ognizes the indispensability, for any kind of learning, of prior biases and affinities" (Quine 1970).

It is highly significant that, in *Word and Object*, Quine suggested that the best exemplar of the radical translator is the child acquiring language skills, for herein lies the key to the nativist view of language acquisition: the child is construed as engaged in a "translation" process (a "mapping" problem) that would be radically underdetermined were it not constrained by innate concepts or word-learning biases. But what if we should abandon the Cartesian premise on which this picture of language acquisition as a "translation" process rests, that meaning and reference are mental phenomena, and instead view the child developing language skills as *learning the techniques required to engage in different kinds of practices*? The child develops communicative intentions within a richly structured interactional context which involves a "continuous unfolding of individual action that is susceptible to being continuously modified by the continuously changing actions of the partner" (Fogel 1993:29). In such a non-Cartesian view, the child is seen not as *inferring* what adults mean when they use such-and-such a word but, rather, as participating in joint activities that involve a continuous process of mutual adjustment. The child acquires through this co-regulated process the skills *that we describe as "language skills"* (Shanker and King n.d., Savage-Rumbaugh, Shanker, and Taylor 1998, Hymes 1974).

For example, the child makes a certain arm movement while the caregiver is saying, "Do you want to be tickled?" Eventually sounds or words are introduced (e.g., the child says "tickle"). The word "tickle" is introduced in the context of and becomes an integrated part of the tickling routine. The *meaning* of the gesture or the utterance is constituted by the role that it plays in this routine. That is, the gesture or utterance in this context becomes a *criterion* for saying that the child wants to be tickled rather than *evidence* of some "hidden mental state" (the "desire to be tickled"). It is on the basis of the child's behaviour (gestures, response to being tickled, repetition of the gesture as soon as the tickling stops, etc.) and the context in which this occurs that one says that the child wants to be tickled. The statement "S is telling you that she wants to be tickled" does not license or rest on the inference that S has experienced some mental state which (*a*) she first identified and (*b*) then chose a word for that she thought would best convey it to her caregiver. Instead, the statement is grounded in this particular language game.

In this non-Cartesian approach, a child is seen as learning how to do different kinds of things with words—learning the rules for participating in different kinds of language games—and *referring* is but one of those things. According to Cartesianism, the child must somehow be able to infer what language-speakers are referring to when they use language. In contrast, the child is here seen as gradually learning how to participate in different kinds of social practices (e.g., giving and requesting objects, playing peek-a-boo, asking and answering simple questions, etc.). With increasing mastery of these practices the child is progressively described as "intending or trying to do such-and-such," "looking or hoping for *x*," "thinking or believing *p*," and so on. Gestures, utterances, and actions serve as the criteria for what the child thinks or means or understands.

Thus, one of the central questions that linguistic anthropology raises is not whether field research provides us with any evidence about how a child might get from a null cognitive state to language competence (with or without any appeal to abstract concepts or unique language-learning biases), but, rather, whether cross-linguistic studies provide us with a completely different perspective on the (culturally variable) processes involved in language development. Instead of asking how children acquire a free-standing, decontextualized communicational system, linguistic anthropology is asking what sorts of things children learn when they learn how to engage in the culturally significant actions that make up their community's "form of life." To illustrate the significance of this shift I will look, in the remainder of this paper, at the case of proper names, which have long constituted the paradigm of the Cartesian view of referential privacy.

Knowledge of Proper Names

There are several reasons that the case of proper names is so central to the nativist thesis. To begin with there is the issue of universality. The anthropologist Donald Brown (who describes himself as a reluctant convert to nativism) lists proper names as one of the paradigms of UP, the language spoken by his hypothetical Universal People (Brown 1991:133). The implication here is that, although different societies may adopt different naming conventions (for example, what kinds of names are suitable for boys and girls, children and adults, etc.), whatever the language there is always a distinct category of linguistic construct in which "N.N." rigidly designates NN. This is not to say, however, that all societies actually use proper names, let alone use them in the same way. Amongst the Malagasy, for example, there is a strong prohibition against the public expression of proper names (see Keenan 1976). But the fact that different societies may imbue proper names with different kinds of properties does not mitigate the fact that every language has proper names and, according to the nativist, that what one knows when one knows that "N.N." is a proper name is always the same.

Then there is the issue of evolutionary significance. In a fairly standard view, the turning point in human evolution was the emergence of the "capacity to *understand* that entities can be referred to by use of their jointly known names" (Noble and Davidson 1996:224; Davidson 1999). In other words, the transition from earlier human species is said to have occurred as the result of a momentous psychological event that sparked off the extraordinary cognitive and linguistic development witnessed in *Homo sapiens*. The mental event in question was the sudden realization that gestures or sounds could

be used not just to draw attention to something present before the speaker but to *name* that object. In other words, hominids are said to have crossed the "language Rubicon" when they realized that gestures or sounds could be used as *symbols* that referred to spatiotemporally distant items and events or applied in novel circumstances (King and Shanker 1997).

This "discontinuity" view of the emergence of the modern mind and the origins of language is frequently backed up with various autobiographical memoirs documenting the profound effect on cognitive and linguistic development that occurs when a non-linguistic adult suddenly experiences the so-called naming insight. Perhaps the most famous of these accounts is Helen Keller's description of the moment when she grasped that Miss Sullivan was spelling out the word "water" on her hand.[7] Discontinuity theorists also cite the transition that infants make (generally between the ages of 10 and 13 months) from proto-words to first words. Despite all the evidence that this transition is gradual and that there is no clear demarcation between prelexical forms and "true words" (Bates et al. 1979), the discontinuity theorist argues that, insofar as this transition rests on the child's first experience of the "naming insight," when it occurs it is both sudden and complete (like an aspect shift).[8]

The term "naming insight" alludes to the so-called Eureka phenomenon, named for Archimedes' exclamation on his discovery that the volume of an irregular solid could be measured by the displacement of water. It refers to the psychological experience that is said to occur when an infant first realizes that certain sounds or gestured are *names* of objects (McShane 1979). Hitherto the infant has performed basic communicative functions using proto-words, "phonetically consistent forms" which are highly idiosyncratic and context-bound (Halliday 1975). But all of a sudden the infant grasps how *names* as such are "mapped onto a representation of objects, actions, events, and attributes in the environment, not merely paired with a particular event" (Adamson 1995: 169). Once this "mental act" of *referring* has been experienced, the acquisition of language proper can take place.[9]

Putting all these factors together, we can see why proper names play such an important role in the Cartesian view of language. Proper names constitute a paradigm for the mental act that is thought to occur in an act of reference *per se* (Wittgenstein 1953). Nouns are seen as names of very different kinds of things. Proper names are the names of individuals belonging to such natural kinds as people, places, and animals. Lévi-Strauss sought to define proper names as the limiting case in any system that individuates the members of a class (and all human societies individuate their members, even though some societies may do so with an ordinal system of proper names or with birth-order names or may wait several years until a name becomes "vacant" that can be given to the child). In this schema, proper names are said to represent the class of individuals "at their most modest" (Lévi-Strauss 1962:197); in many cases they serve as a sort of place marker until one of the other kinds of name (e.g., ceremonial names, kinship terms, necronyms, nicknames, patronyms, teknonyms, titles) can be assigned. The problem with this argument is that there are countless cases in which an individual's proper name entails obligations to the natural and/or the social environment (see Wagner 1972). But what Lévi-Strauss was driving at is that, while proper names are not the central linguistic phenomenon that 19th-century philosophers had assumed, they nonetheless constitute the paradigm of *referring*—the pure mental act involved in knowing that "N.N." refers to NN, which is always the same, in every human mind, regardless of the kind of thing named or the various cultural properties associated with names.

The view that proper names are *rigid designators*[10] can be traced to John Stuart Mill's argument that proper names refer as the result of an arbitrary connection established between a name and its referent and have no associated (connotational) meaning (Mill 1843). Gottlob Frege challenged this view of proper names in "Sinn und Bedeutung" (1970[1891]). He argued that Mill's view of proper names could not explain our ability to understand sentences that contain an "empty name" (e.g., the name of a fictional character). Nor could it explain the fact that non-trivial identity statements can convey meaningful information, for according to Mill's conception of proper names saying "Hesperus is Phosphorus" would be no more informative than saying "Hesperus is Hesperus."[11] Thus Frege set out to show that proper names must have both a sense and a reference. His conception of what the sense of a proper name might consist in is notoriously vague (he refers to the sense of a name as the "mode of presentation" of an entity), but in essence he seems to have regarded it as the value of a function for an argument (i.e., as a definite description). His goal was to establish that one understands a sentence if one knows its structure and the meaning of each of its constituents, and therefore it was essential for him to show how the sense of a proper name contributed to the overall sense of the sentence in which it was embedded (Baker and Hacker 1984*a*).

7. "Suddenly I felt a misty consciousness as of something forgotten—a thrill of returning thought; and somehow the mystery of language was revealed to me" (Keller 1990:16; cf. Schaller 1991).
8. For example, Alan Kamhi reported that "at 6 p.m. on the evening of 22 February [his daughter] Alison realized that words could be used to name objects" (Kamhi 1986:159).
9. In Kamhi's words: "Alison had finally learned that words could be used to refer to concepts independently of her communicative experiences with these concepts. Once she realized this symbolic property of language, she solved the problem of genuine reference and began to produce her first words" (1986:159).
10. A proper name is said to be a rigid designator in the sense that it refers to the same individual at all times and in all situations (including counterfactual conditions), regardless of who is using that proper name or when and where that name is being used (see Kripke 1980).
11. The argument refers, of course, to the discovery that the so-called evening star and the morning star were one and the same, the planet Venus.

The questions raised by Frege about the semantics of proper names were at the forefront of the philosophy of language throughout the 20th century (Baker and Hacker 1980). The debate was essentially over whether a proper name has a meaning consisting of a uniquely identifying description or cluster of descriptions or is a rigid designator. But despite these disagreements over whether the meaning of a proper name consists in a mental representation of the person it denotes or a set of identifying features of that person, all Cartesian theories have accepted that referring is a *mental act* as opposed to something that *agents* do when they use words to perform speech acts. Moreover, according to the Cartesian view, mutual understanding occurs—if and when it occurs—when language-speakers associate the same representation with the same name. Hence it follows, given the indeterminacy of reference, that there *must* be constraints operating if a child is ever to master the use of proper names.

The reasoning underlying this nativist conclusion is as follows: In order to know that "N.N." is a proper name, children must know that it picks out a unique individual, which presupposes that they know that that individual is a member of a particular kind (Macnamara 1986). To strengthen the view that this knowledge of the function of proper names is innate, the Cartesian stresses that children are not explicitly taught that "N.N." is a proper name or how to use proper names; rather, they are said to know intuitively that "N.N." is a proper name (Macnamara and Reyes 1994). Thus the Cartesian argues that "if you show a young child a photo of 'Eric the Red' in a book, the child instinctively grasps that 'Erik the Red' is all one name, and what's more, that it is the name of the person in the photo and not the name of the photo itself" (Macnamara and Reyes 1994). Moreover, children seem to be instinctively attuned to regard certain sounds in certain situations as rigidly designating persons. This fact is especially noteworthy, according to the nativist, when one considers how many different aspects of NN there are to which "N.N." might be referring.

For example, we might say to a child, "That's Red," referring to the colour of a man's hair or his sweater or to the man himself. How does the child know, in the latter context, that "Red" refers to the person and not to the colour of his hair or his sweater? And how does the child know that another man with red hair or a red sweater is not automatically called "Red"? Or that all other men are not called "Red"? And how does the child know, on encountering Red a few years later, when he has gone bald, that he is still called "Red"? And, supposing that NN was sitting the first time and standing the next, and so on, how does the child know that none of this has any bearing on what NN is called? (Suppose his name was "Sitting Bull.") For that matter, how does the child single out the sound [red] from the continuous speech stream [ðaetzred]?

The Cartesian argues that the open-ended nature of the myriad ambiguities with which children are confronted when they hear adults using proper names reveals a crucial fact about the built-in constraints that *must* be governing their cognitive and linguistic processing:[12] in order to know that "N.N." designates NN, they must know the category to which NN belongs and the grammatical category to which "N.N." belongs (Macnamara 1986). Children cannot know that "Red" refers to Red unless they see what *people* have in common and therefore see the *person* Red and not some part or attribute of Red; and they cannot know that "Red" is a proper name unless they know how to combine proper names with other symbols.

There are thus two interrelated themes underlying the nativist conception of proper names. The first is the Cartesian premise that, in acquiring language, the child's mind/brain is confronted with the problem of mapping words onto mental representations. The point of the sceptical argument about the radical indeterminacy of reference is to establish that, without innate cognitive biases, a child would never be able to map the right names onto the individuals that they designate. The second part of the argument is that to know that "N.N." is NN's *name* the child must know the grammatical role of proper names in sentential constructions. Thus the Cartesian concludes that not only are proper names the same thing in every language but, further, to know what a proper name is *is the same thing in every human mind*. In other words, children are born with the implicit knowledge that "N.N.s" are used to designate NNs, and all they have to learn is particular "N.N."–NN pairings and the conventions adopted by their society for publicly using "N.N."

But what if a child uses "N.N." correctly, according to accepted social customs, but thinks that "N.N." refers to some part of NN and not to the person NN? What if the child does not even possess the concept *person*? This would mean that, despite having mastered all of the appropriate social conventions for using proper names, the child nonetheless does not understand that "Red" is a proper name. This incongruity is precisely the point of the behaviourist conclusion that Quine sought to draw from his argument about the radical indeterminacy of translation. The upshot of Cartesian epistemology is that the classification of a common noun as a proper name depends on the mental representation that the agent associates with that term; the manner in which the word is used is said to be irrelevant to its status as a proper name. Thus, even if the child should say that "Red" is Red's name, use "Red" appropriately when greeting or calling him, etc., this would not *ensure* that the child was using Red as a proper name; these social rituals might simply have been conditioned or memorized.

In terms of the Cartesian conception of reference, we

12. The argument here is that, insofar as there are so many different things that people might be referring to when they use a name, children must be "equipped with some assumptions about the nature of categories and about the nature of category terms. These assumptions limit the kinds of hypotheses children consider. In other words, children do not always need to reject hypotheses [about the reference of a name] on the basis of negative evidence. They can implicitly reject them by being biased against them" (Markman 1989:7).

have no way of knowing for certain what referent a child is associating with a term and therefore can *never* know for certain whether the child is using a proper name. Indeed, language-speakers can never know for certain whether they are both talking about a person, let alone talking about the same person. And it is precisely here that nativism enters, not primarily as a way of explaining the genesis of linguistic knowledge but, rather, *as a way of blocking the sceptical problems that plague Cartesian semantics*. Nativism assigns the relevant cognitive and grammatical categories—the built-in constraints described above—to the hard-wired machinery of the mind. Thus children have no choice about what kind of thing they will correlate with "N.N.": the mental association between *persons* and *proper names* is said to have been biologically constrained. The psychological leap that our hominid ancestors are said to have experienced when they suddenly grasped the representational function of names was naturally selected, encoded in the human genome and imprinted on the human mind/brain.

But what if we were to adopt the view that such actions as the ability to answer the question "What is his name?", to use "Red" appropriately in greetings and farewells, etc., *justify* describing the child as "understanding that 'Red' is Red's name." This move has far-reaching implications for our understanding of the nature of proper names and the development of the ability to use proper names. Gone is the assumption that the meaning of a proper name consists in a mental representation of the person it denotes or a set of uniquely identifying features of that person. Gone too is the sceptical problem about the indeterminacy of reference and, with it, the demand for built-in cognitive and linguistic constraints to explain how language-speakers succeed in understanding one another (Taylor 1992). And finally, gone is the temptation to treat the naming insight as a mental experience; instead, we compare the ability to use proper names to the development of other, more prosaic abilities.

After all, there are countless occasions when one suddenly masters a skill in a way that can be described as a "Eureka phenomenon." There comes a moment in tennis, for example, when after many fruitless hours of practice and instruction one suddenly "feels" how to hit a top-spin forehand. But one would hardly explain this moment as the result of a mental experience. In this case, the exclamation "Now I know how to hit the shot!" does not report on an "epistemic state." Nor does it describe the feelings we might have experienced at that precise moment, even though it is unlikely that we would have said this had we not experienced a sudden rush of elation at the distinctive sensation of "brushing up" on the ball. Rather, we might use this expression to express our conviction—which, in far too many instances, is unwarranted—that we shall henceforth be consistently able to execute this stroke (Shanker 1998).

What if learning how to use proper names is similar to learning how to hit a difficult tennis shot, insofar as this is also a skill that a child can only master when more primitive skills have been attained? To be sure, the rules involved in language learning are of an order of magnitude that strain the analogy to tennis. But what is interesting in the present context is simply that there are instances in which this skill may be acquired quite suddenly but that, more commonly, it takes some time before the child can be said to have mastered it. In the majority of cases, a child continues to use prelexical forms alongside true words, and it may not be until around the age of 18 months that the latter finally start to predominate (Bates et al. 1979). "A general indication of the initial difficulty that the child has in coming to terms with what it is that words do is the fact that the learning of first words is a very slow process: up to 5–6 months may elapse between the production of the first word and the production of the tenth" (Harris 1992:70). But the reason this skill is so difficult to acquire is not that the child's mind is having so much trouble making the leap to "referentiality": it is that it is difficult for the child to master the use of conventionalized sounds (or gestures) in contexts other than the situation in which they were first acquired.[13]

Thus, instead of postulating an abrupt transition from prelexical forms to first words that is brought about by a nebulous mental experience, this approach considers the ability to use names as the result of the child's cognitive development and mastery of various communicative and prelinguistic skills (see Tomasello 2001). According to this non-Cartesian view, "while naming is an important development in language learning it is not the first," for "naming is a fairly advanced function of language, presupposing a level of attention on objects and other phenomena that requires a certain level of social development" (Beaken 1996:69). In place of the maturational view of the child's transition through language milestones that are genetically predetermined, this model stresses that before mastering the use of names the child has to go through such developmental stages as engaging in joint attention (which emerges around the age of 2 months), turning the head to see where a sound is coming from (around 3 months), shaking the head to indicate "no" (6–9 months), using gestures to request things (around 9 months), intentionally pointing at things (around 12 months), using sounds and then first words to coordinate and initiate joint routines, and so on (Shanker n.d.*a*).

In other words, what linguistic anthropology shows us is that, once language development is viewed in non-Cartesian terms, it becomes apparent that much more is involved than just acquiring a repertoire of interactional techniques; more fundamentally, language development involves enculturation into a community's distinctive way of being-in-the-world (Basso 1988). From a non-Cartesian perspective, the reason Quine's radical indeterminacy of translation has the force that it does has nothing to do with the epistemic privacy of reference

13. This is very much the problem faced by speech-language pathologists who must try to assist children suffering from language deficits to generalize what they have been taught in therapy sessions to novel situations.

(given that "referring" is something that agents do with words); rather, the source of the ethnographers' dilemma is their social and psychological distance from the "form of life" that they are seeking to penetrate (Agar 1996).

In this respect, the position in which ethnographers find themselves can indeed be likened in some ways to that of children developing language skills, for in mastering a language one is learning ways of behaving that *count*, within a community, as the performance of some culturally conceived act (Shanker and Taylor 2001). But then, such a comparison highlights a crucial respect in which learning how to use proper names is *not* like learning how to hit a tennis shot. Children cannot be said to have mastered the use of proper names unless they know what constitutes a proper name *in their linguistic community*, and this last clause is precisely the point that is denied by the Cartesian nativist account of proper names.

Hence the question we are ultimately concerned with here is the significance of abandoning the Cartesian epistemological assumption that what one knows when one knows that "N.N." is a proper name is logically distinct from how one's community uses proper names. What if what varies from culture to culture is the very things that children must learn when they learn what constitutes a proper name for their community? If that is the case, then cross-linguistic studies of naming practices are crucial for our understanding not just of the concept of proper name but, indeed, of language development in general. Such research not only provides us an opportunity to look more closely at the kinds of skills that children are mastering when they learn how to use words but, more important, forces us to shift our focus from viewing the child in the same way that one might view a computer program—viz., as equipped with certain heuristics that enable the mind/brain to extract natural kinds from visual input and proper names from verbal input—to seeing the child as first and foremost a developing social agent in a familial and a larger cultural environment (Ochs 1988).

Learning How to Use Proper Names

Psycholinguistic studies on the development of children's ability to use proper names have been largely (if not exclusively) confined to the Anglo-American context. This research indicates that one of the first steps that children take in learning how to use proper names is learning their own names. This ability begins to emerge remarkably early: infants typically begin to recognize their own names around 4 months, which is far in advance of the cognitive milestones that one might have thought were prerequisites for name recognition (e.g., self-recognition, which, judging by the mirror-recognition test, does not emerge until around 15 months). Does this discrepancy between cognitive and linguistic abilities constitute further evidence that children possess innate knowledge of the construct *proper name*? Or does it simply reflect caregivers' much more frequent use of those names than any other word when interacting with them?

The case of name recognition is especially interesting because it is a step that many would argue is taken by several animal species as well as humans. Rigorous testing has demonstrated that the chimpanzees Washoe, Loulis, and Ai, the orang-utan Chantak, and the bonobos Kanzi and Panbanisha know not only their own names but the names of other apes in their compound and the names of the caregivers who work with them (see Gardner and Gardner 1969, Matsuzawa 1996, Miles 1994, Savage-Rumbaugh et al. 1993).[14] And many would argue that, for example, dogs and cats also know their own names. But this very continuity argument should alert us to the dangers of overinterpreting children's earliest reactions. Speech experts have shown that what, for example, a dog is responding to when its name is called is not the name per se but stress patterns in the vocalization. Hence the question that psycholinguists have looked at is whether children are similarly responding to stress patterns instead of recognizing their names (Golinkoff and Hirsh-Pasek 1999).

To answer this question, scientists have used a head-turn procedure to see if children respond differentially to their own names, different names with the same stress patterns, and different names with different stress patterns. On the basis of these tests it appears that children progress through stages as they gradually begin to recognize their own names. At first they react to their names in much the same way that one might react to a doorbell: beginning around the age of 2 months their faces light up at the sound of their names, but here they are responding to the sight of the caregiver, which becomes associated with the sound. Judging from the head-turn test, they do not begin to recognize the distinctive sounds of their names until around the age of 4 months. They do not actually begin to turn and look expectantly in the direction from which their names are being called until around the age of 6–7 months (Golinkoff and Hirsh-Pasek 1999:52–53). This too is a behaviour that may appear quite suddenly or may gradually phase in.

At this stage we are dealing with the communicative techniques that caregivers use to engage infants' attention. Infants' responses to their names are entirely bound up with their anticipation of effective interaction (a point which clearly has important implications for the second of the "universals" noted in the opening section). But, of course, even once they have begun to recognize their own names they are still a long way from understanding "N.N." as a *name*. Even if they respond immediately when their names are called and, further, respond appropriately when asked their names, this may not in itself constitute sufficient evidence that they know what their names are. After all, a parrot can perform much the

14. I am indebted to Steve Wise for drawing my attention to Janik's (2000) report that bottlenose dolphins imitate the learned whistles of other members of their group, apparently in order to address those individuals. Such a finding may have interesting implications for how we think about the origins of naming practices.

same feat, as can a computer program. This is a point that Chomsky (1959) was making in his review of Skinner's *Verbal Behavior*—that merely being *conditioned* or *programmed* to respond to such-and-such a sound does not constitute evidence that S *knows* that "N.N." is its name. But far from supporting a nativist epistemological conclusion, this is the *logical* point that a subject cannot be described as *knowing* what its name is unless the subject possesses the *relevant social and linguistic knowledge*.

But then, what exactly is the nature of such knowledge? The crux of the non-nativist response to this question is that it cannot be separated from the developmental and the cultural context in which it is asked. The criteria for attributing knowledge to a child are highly sensitive to that child's stage of development. (For example, we would not expect the young child to know that proper names are capitalized.) Furthermore, the criteria for describing children as knowing their names cannot be divorced from the cultural context in which this knowledge is attributed. Lévi-Strauss (1962) has described a continuum of social naming practices: at the one extreme are those cultures in which individuals are named by applying rules that fix the child's position in a preordained class or group and, perhaps, formal rules for acquiring a new name at prespecified junctures in one's life, and at the other extreme are those which assign a minimal amount of social information to the name bestowed on a child and have no rules for assigning new names according to various rites of passage or the death of relatives.

To see how this principle operates, we might compare naming practices in Anglo-American culture with what Navajo children must learn when they learn what a name is. The Anglo-American practice of naming[15] falls at the "modest" level in Lévi-Strauss's scheme. To describe a female child in Anglo-American culture as knowing that "Leila" is her name, for example, we might require that she know such things as that everyone, regardless of age or sex, has a name, that "Leila" is a girl's name, that one's name is composed of a first name, a family name, and possibly one or more middle names, and that siblings have the same family name (typically taken from the father). As Leila grows older she will be expected to know which name to use to greet someone according to the context (e.g., that it is inappropriate to address people by both first and last names or solely by their last names). But even when she is older she will not be expected to know all the things that are expected of an adult, for example, whether to use a first name for someone or a formal title (with either the first or the last name), that in some instances it is permissible to address people one has just met by their first names and—in the United States but not in Canada—to shorten that name immediately to a familiar contraction, that it is a sign of intimacy to address people by their nicknames but one cannot presuppose intimacy by bestowing a nickname on someone one is meeting for the first time, and so on.

One of the keys to describing Anglo-American naming as an exemplar of a "modest" practice is the manner in which names are chosen. It is important to be clear here, however, that so describing the Anglo-American practice does not signify that there is not a tremendous amount of importance assigned to naming. On the contrary, a child is given a first name as soon as possible after birth (usually by the parents), and we treat it as an important sign of psychological distress if a caregiver displays no interest in naming the child. Children are generally given two or more names, but only one will be selected as the child's "given" name. Young children are rarely called by their full names; more often, parents will use a diminutive of the child's given name, a pet name, or a term of endearment.[16] It used to be the case that children were named after a deceased relative or an important religious or political figure, but it is more common these days for them to be named after an actor or an athlete or to be given one of the currently fashionable names. Interestingly, baby naming has become a commercial concern; there are self-help books that consist of lists of possible names, and polls are regularly conducted to chart current naming trends.[17]

The possible meaning of a name (e.g., "Grace," "Charity," the fact that "Leila" means "night" in Hebrew, etc.) has become increasingly irrelevant to its adoption in Anglo-American culture. Very rarely does one hear of a child's being named because of a resemblance between the child and something else (e.g., a natural phenomenon or another person). It still remains the case that children are named after important figures, but this seems to constitute more of a statement on the part of the parents than a belief that by being so named the child will come to share the character traits of that figure. Names are rarely taken out of circulation following someone's death (although, in an interesting twist on this practice, a player's number in professional sports is frequently "retired" to honour a great athlete). In some families, particular names are regarded as special family possessions and are passed down from one generation to the next. Despite the considerable freedom that parents now enjoy in choosing names, they still continue to observe many constraints; for example, they are fairly careful to dif-

15. One might object here that we run the risk of reification by assuming that one can speak without qualification of "the Anglo-American practice of naming." Yet one might also argue that the surprising homogeneity that one finds in Anglo-American naming practices is precisely the sort of factor that leads one to want to speak of such a thing as "Anglo-American culture" (see Shanker n.d.*b*).

16. Children quickly learn the significance of being called by their full names. It is interesting that this usage should have become so widely adopted in Anglo-American culture as a way of expressing anger or foretelling punishment. In his account of the Western Apache "speaking with names" Basso (1988) stresses that the practice hinges on using place-names in their full form rather than the usual abbreviated renderings.

17. At the opposite end of this particular spectrum would be the Balinese practice in which infants' names are arbitrarily coined nonsense syllables which have no family connections and are not duplicated within a community (Geertz 1973).

ferentiate between "girls"' and "boys"' names and to stick to familiar "American"-sounding names, and there are clearly societal limits (e.g., recall the commotion when the singer Grace Slick tried to name her daughter "God").

In Anglo-American culture, as, apparently, in all cultures, one's personal identity and, to some extent, one's self-esteem are closely bound up with one's name. Deliberately mispronouncing or punning on someone's name is frowned upon, and complex libel laws have established strict guidelines about the kind of liberties one can publicly take with someone's name. Writing one's name on different kinds of document and, in some contexts, just saying one's name aloud can have binding properties.[18] Certain names can be viewed as an impediment in certain professions, and, conversely, different names can be viewed as an asset. And to reject the name one was given by one's parents in order to choose one's own name is seen as a very significant step requiring formal certification if it is to be recognized by the state.

Few things are valued as highly as having one's name live on after one's death, and on rare occasions one will live to see one's name "immortalized," for example, by being used to describe a personality trait or conduct ("Churchillian") or name a school of thought ("Wittgensteinians"), or prefaced with a definite article ("The Donald"). Changing one's title (e.g., becoming "Dr." or "Professor" or "CEO") literally changes one's social identity.[19] All sorts of subtle messages are conveyed by the name one is called in certain situations (e.g., being called by one's first name in the doctor's office) or the name whereby one introduces oneself. And without doubt, one of the most dehumanizing things one can do to others is to strip them of their names entirely: hence the use of numbers rather than names to refer to prisoners and, for that matter, the use of "dog tags" in the armed services or terms of rank rather than names.

The Navajo practice of naming provides an illuminating contrast with the Anglo-American because it lies much more towards the socially structured end of the spectrum. Robert Young noted in 1961: "The personal name did not formerly function as an instrument for general identification purposes among the Navajo as it does among non-Navajos." According to Young, a baby was given a "war name" by a close relative who had been to war. This name was a somewhat stereotyped phrase describing a warrior's activity (for boys) or raiding (for boys and especially for girls). It was not intended for everyday use but rather was restricted to ceremonies such as the many healing and blessing rituals.

Young was describing a practice that predated 1960 and probably extended much farther back in time—certainly to the 1800s, when Navajos were active in war and raiding, and possibly earlier. In these pre-1960 practices, as the infant grew he or she would be referred to directly as Son (Shiyaaz) or Daughter (Shizei), Grandson or Granddaughter. This firmly established the child in the network of kin and clan. The child also became known by a descriptive name such as Big Baby, Big Boy, Grey Girl, or Laughing Boy (Young 1961:539).

As children grew into adults they continued to be referred to by kin terms, as well as by a series of descriptive, occupational, or role terms (cf. Wagner 1972). These descriptive names would often have a little twist to them, manifesting the Navajos' love of words and word plays. Young's examples (which he gives in English translation) include The Nephew of Speckled Horse, Tall Salt Clansman, Plump Woman, and Deaf Woman. Others would include Atsidi (Smith) and Chischilli Begay (Curly Hair's Son). Men would also be referred to as So-and-So's Husband and women as So-and-So's Wife, and if the marriage broke up these same individuals might be referred to differently. A name usually reflected a specific attribute or role and could change in accordance with changes in the individual's life or person. Outside one's community in a strange but still Navajo environment, one would introduce oneself by one's clan, for example, "I'm To dich'ii'ni', born for Bit'ahnii." If one remained in this new environment, one would receive a new descriptive name.

The influences and pressures from outside society are reflected in Navajo naming practices in interesting ways. From early on, when the Southwest was first a Spanish outpost and then part of Mexico, many Navajos had Spanish names. These would have been regarded as additional names, specifically used with Spanish-speaking people. A Navajo's Spanish name would probably have been similar in kind to other names and would have changed from time to time as the groups with which a Navajo was affiliated or working with changed.

After 1848 the pressures and requirements of the now dominant Anglo government and American culture in general began to have a significant influence on Navajo naming practices. Schools for Indian students—both the day schools of the early 19th century and the off-reservation boarding schools for Indian children from all tribes that were established between 1879 and 1900—regularized Indian names, if only to make pronunciation easier for English-speakers. Terrified children who spoke little English found themselves confronted by Anglos demanding that they give their names. Even if they understood the request they were unlikely to comply, for they had usually been taught not to reveal their names. Employees at schools for Navajos and the agents of the Bureau of Indian Affairs therefore gave Navajo children names that conformed to Anglo naming con-

18. A fascinating example of the kinds of social acts performed with proper names can be seen in the use of kinship terms in Chinese marriage: "The essential transformative moment occurs—not, as in the US, in an overt performative statement *I do* [agree to be and am hereby married]—but when she speaks the kinship terms appropriate for a husband and wife to use. She becomes a wife by uttering the terms for his relatives" (Blum 1997:362).

19. In certain ethnic groups a child may be given two completely different names. For example, until recently, a young Jewish child of European descent always received both an English name and a Yiddish name. This could be a very significant issue for orthodox Jewish children, who would frequently see themselves as having two completely different identities—as moving in two completely different worlds (and adopting different personalities for each of these worlds).

ventions—a "first" name and a "last" name. These names sometimes incorporated the descriptive or role name of the Navajo individual, usually Anglicized. The children had to accept whatever names the teachers had given them, for example, the name of an American president or general or some other well-known figure or even the name of the teacher or principal of the school. But the Navajos probably viewed a name like this as just another term to be used in specific situations.

The practice of designating Indian people by census number began in the 1928–29 Census and arose from the difficulties the non-Indian officials had with Indian names. As Anglo traders, missionaries, and medical personnel came into increasing contact with Navajos in the 1930s and as New Deal programs began to provide small cheques for labour, names (including Navajo words used as names) were increasingly formalized by the non-Navajo culture. Influenced by Anglo practices and record keeping, Navajos began to be identified by one set of names used over time. Slowly, the habit of extending one particular name (or phrase, or combination of names) to one individual and related names to others in that individual's family began to become more common among the Navajos.[20]

Young also reported that Navajos were sometimes "named by somewhat sadistic non-Navajo associates or acquaintances . . . Popeye, Angel Whiskers, Trixie Calamity," and other such names, some "unprintable" (1961:540). Navajos themselves gave Navajo descriptive terms to Anglos they were acquainted with, such as traders or agents, and these too reflected physical characteristics and kin terms. Some of these terms (translated into English) were Big Tooth, Big Tooth's Son, Many Horses, and Angry Woman. Descriptive terms for traders often referred to ears (Crystal Ears, Little Round Ears) and, on the few occasions when a name was explained, seemed to refer to the inability of Anglos to understand either the Navajo language or a specific situation.

When we compare Anglo-American and Navajo uses of proper names we can clearly see a number of common themes. The most obvious is that names are used in both cultures to refer to individuals and that everyone, regardless of age or sex, has a name. In each culture there are customs and conventions for naming children, and names are used to greet one another. What is much more striking than these commonalities, however, is the differences in the conventions and, more important, the cultural learning of children in relation to their names. A description of the way in which names are used among Navajos captures not only some striking conceptual differences but also the element of *change in practice over time*, which needs to be recognized as much in naming as in other linguistic acts. Indeed, even the simple act of greeting is significantly different in the two cultures (Hall 1994:89–90):

> White males (and now most white females) grip the proffered hand firmly . . . looking the other party directly in the eye, all of which is intended to convey interest, honesty, and sincerity. But when we would do this to the Navajos, it conveyed quite a different message; a direct, unwavering gaze meant anger. . . . The Navajo greeting does not center on showing relative strength and dominance (as it does with two Anglo males) but is instead a communication in which there is a mutual assessment of feelings and expression. As two men approach each other, eye contact is broken—at about the point where it is possible to begin to pick up the details of facial expression. Once this boundary is crossed, they look past each other, holding the approaching figure in their peripheral field of vision. To look directly at the other is tantamount to swearing at them.

And, as we have already seen, announcing one's own or using the other person's name would, in fact, be carefully avoided.

One of the most striking differences is the Navajo use of a "ceremonial" name, which would be known only by a very few and revealed only to those who were most trusted. Describing Navajo culture in the 1930s, Hall (1994:26) remarks: "To a Navajo, a person's name is sacred and is endowed with power; they do not abuse that power by calling people by their names to their faces." Moreover, the ceremonial name was not supposed to be told to others for fear of misuse or even witchcraft. Hence it was imperative that children learn as early as possible the difference between their ceremonial names and descriptive or kin terms. Furthermore, they had to learn that it was very rude to ask someone directly, "What is your name?" And one of children's earliest lessons was that one must never mention the name of the recently deceased.

Another striking difference is the absence of surnames in the pre-1960s naming practices. In Anglo-American culture, the bond between siblings is cemented by the family name, which derives from the father's line. But in Navajo society the kinship relationship between siblings is based on their uterine bond (which is a further reflection of the intensity of the mother-child bond in Navajo society). Witherspoon (1975:31) reports that "it is the marriage of the father to the mother which ties the father to his children. When the marriage is dissolved, the father-child relationship is behaviourally and functionally dissolved, or almost so." In place of the significance assigned to the father's surname in Anglo-American practice, what really marked Navajo individuals, and what children had to learn very early on, was the complicated matter of their clan affiliation. This affiliation came from the mother, but she was born for her father's clan and had in addition the clan affiliations of her maternal and paternal grandfathers. The importance

20. Young gives examples of Navajo words that became formalized as names by outside usage: *hastiin* (meaning "man," often used as a first name); *yazhi* (Yazzie, Yazzy, etc., "little"), *biye* (Begay, "son of"), *atsidi* (Atsiddy, Etsiddy, Atcidy, etc., "Smith"), *neez* (Nez, "long," often used as a last name either in Navajo or sometimes in translation [Long]). It was common to encounter a man named Hastiin Begay (literally, Son of Man) or a woman called Asdzaani Nez (Tall Woman).

of these kinship ties for the individual was paramount: one might not marry into one's clans (which was considered incestuous), and one was under considerable moral obligation to one's fellow clan members to provide food and other subsistence items and protection.

In general, there was no Navajo "full name" comparable to the Anglo-American one that would be used to refer to individuals over the course of their lifetimes; rather, Navajo names were situational, descriptive, and role-related and often changed over time. Furthermore, individuals were identified amongst their relatives or when meeting strangers by a series of identifying words, the most important of which were their clan affiliations. Until about the mid-1960s the Navajos gave whichever of their "names" the situation required or changed to a new "name" if this seemed more appropriate, presenting the non-Navajo culture, accustomed to the idea of "one person/one name," with a sometimes serious communication problem.[21]

Although Navajos are in no doubt as to who they "are" (the individual has both responsibilities and independence in Navajo culture), in the past this individuality was not marked by a single name. In early Navajo society, infants would learn their identities as sets of relationships with other people, and their "ceremonial" names were reserved for special use by close relations and in sacred ritual. Navajos now use Anglo naming conventions—a first and last name and sometimes a middle name—and use their names, as well as the names of others, in reported speech in much the same way as in Anglo culture. But many Navajos still have special names that are used in ceremonies and are not generally known except by family members and trusted friends.

When we review these differences between Anglo-American and early Navajo naming practices, it becomes increasingly problematic to suppose that the essence of what children know when they know what a proper name is, is that "N.N." refers to NN and that it is this "knowledge" that enables them to grasp what people are referring to when they use a proper name. Rather, what children *learn* is how proper names are used to manage social relations and, in those societies where this applies, the place that the name marks in the social hierarchy.[22] Thus, what distinguishes children's knowledge of proper names is not some underlying cognitive scheme or linguistic form but, rather, their knowledge of the distinctive role that proper names play in their community's social practices. But to come to terms with the full implications of this remark requires a dramatic reorientation in our attitudes towards the significance of proper names.

21. For example, in the late 1960s and early 1970s, when Navajos began to receive general assistance from state and federal sources, there were instances in which it was not always clear to whom the cheque should go (e.g., when there were two or more Sam Begays or Alice Yazzies living in the same community).

22. This point is especially clear in Chinese naming practices, where individuals are always highly conscious of which name they should use in speaking to someone of higher or lower status. A senior may use a junior's name, but the junior may only use the proper kinship term to address the senior, and under no circumstances should a person be addressed with a pronoun. Thus children are learning not only how to address their seniors but what their social status is, and when they adopt this naming practice they are willingly assenting to their place in this social structure (see Blum 1997).

The Significance of Proper Names

To challenge the Cartesian view of proper names is not to challenge their cultural significance, for there is something arresting about the fact that every society attributes so much importance to assigning names to children. Moreover, the possession of a proper name seems to be integral to a child's developing sense of personal identity. Indeed, so important is this phenomenon that it has understandably led some to wonder whether some sort of Rubicon separating human from non-human was crossed when hominid societies started using proper names.

As far as concerns the nativist view of the internal constraints that must underpin children's capacity to use proper names, however, the critical point is that children cannot be said to know what their own or others' names are unless they know what they are doing when they say what their names are, and to possess this knowledge entails knowing how proper names are used *in their culture*. In more general terms, therefore, what children learn when they learn how to speak is both how to do things with words and *what they are doing with those words*. Hence one of the central points that linguistic anthropology brings into sharp focus is that children's knowledge about language cannot be divorced from the particular *languaculture* in which they are raised (Agar 1994). What the "things" are that children are learning when learning how to do things with words—that is, what behaviour counts as an instance of any one of them—is determined by the reflexive practices of the particular social environment in which they are raised (see Taylor 1997).

Herein lies the appeal of Quine's idea of comparing a child developing language skills to an ethnographer, but, of course, there are fundamental respects in which a child is *not* in the same position as an ethnographer. A child learning how to use proper names is not testing a set of hypotheses, nor are proper names some isolated linguistic construct that a child recognizes as such. The ethnographer may isolate proper names for the purposes of conducting a cross-linguistic study, but from the child's perspective learning how to use proper names is just one aspect—albeit an integral one—of interacting with caregivers, peers, strangers, and others in a socially appropriate way.

In other words, to learn the language of one's society is to learn the sorts of things that are done with words, and this is a matter of learning the values of one's culture (Hymes 1974). For example, one of the most striking contrasts to emerge from our comparison of Anglo-American and early Navajo naming practices is how proper names are bound up with each culture's deepest views about the autonomy of the individual, the ties of

familial relations, and one's larger social and ethical obligations. The Anglo-American practice of naming is inextricably bound up with the heightened emphasis that the culture places on individualism and egalitarianism. To see the force of this point, we might compare Anglo-American naming with Eugene Zamiatin's (1952:7) vision of a dystopia in which proper names have been abolished and in their place citizens are distinguished by numbers assigned to them sequentially at birth:[23]

> The Numbers, hundreds, thousands of Numbers in light blue unifs . . . with golden badges on the chest—the State number of each one, male or female—the Numbers were walking slowly, four abreast, exaltedly keeping step. I, we four, were but one of the innumerable waves of a powerful torrent: to my left, O-90 (if one of my long-haired ancestors were writing this a thousand years ago he would probably call her by that funny word, *mine*); to my right, two unknown Numbers, a she-Number and a he-Number.

The numbering practice of Zamiatin's "United State" shares some of the functions performed by proper names in Anglo-American practice: numbers are used to refer to individuals, and citizens use them to greet one another. But there are no numbering customs, no choice in numbers, no "nicknumbers," no conventions for using numbers in conversations according to varying contexts, etc.

What such a comparison highlights is that, while one of the primary uses of proper names is to refer to people, it is not the sole function, and to reduce the linguistic function of proper names to this referential usage would be to distort what differentiates *proper names* from *labels*. The fact that "O-90" can be used to individuate someone does not mean that "O-90" is *ipso facto* a proper name. What is missing in the numbering system in Zamiatin's *We* is all those elements of Anglo-American or Navajo naming practices that serve to define an individual's identity and that shape how individuals interact with one another.[24]

Thus, were one to argue that what one knows when one knows that "N.N." is the name of NN is distinct from the conventions for using "N.N." (e.g., that what one knows is that "N.N." is a "mental representation" of the person NN), this would entail that what one grasps when one understands the proper use of "O-90" would be no different from what one grasps were that same person to be called by a proper name. And, of course, the whole point of Zamiatin's *We* is to illustrate the fallacy of this assumption—to illustrate the internal relationship between the concept of *person* and the possession of a proper name, which is depicted so poignantly at the end of the novel when the last remaining vestige of D-503's humanity is destroyed by the State and he reverts to using numbers instead of proper names.[25]

The clear implication of this argument, as regards the case of children learning how to use proper names, is that they are learning not how to use labels or markers but how to manage social relations—indeed, how to be *individuals* within their society. But the deeper epistemological point at issue here concerns the basic premise of the Cartesian view of proper names, which is that one must distinguish between the *meaning* and the *use* of a proper name, where the former consists in a mental representation of the person (animal or thing) denoted and the latter consists in the social conventions associated with the name in question. To grasp the *use* of a name is seen as a public activity, which as such is guided by older social partners, but to grasp the *meaning* of a proper name is seen as a private mental experience, which as such must be "guided" by internal mental constraints. From the non-Cartesian point of view, however, the problem is not how children come to have the same mental representation as the other members of their community when they use the name "N.N." but how they come to perform the appropriate social actions governing the use of "N.N."

Instead of assuming an isolated mind and trying to infer the extension of a term, the non-Cartesian approach looks at the manner in which children master the use of different kinds of words in co-regulated activities with their caregivers (Shanker and King n.d.). The epistemological problem which demanded the introduction of nativist constraints to explain how children could possibly come to use proper names simply does not arise, for to grasp the use of a name *just is* to grasp its meaning. Children cannot be said to be using "N.N." *as a proper name* unless they are performing such-and-such a social act, and performing such-and-such a social act *necessitates* that they know what they are doing. Hence to talk about the meaning of proper names has nothing to do with epistemic privacy; rather, it is to clarify the sorts of social acts that agents perform when they use proper names.

In other words, an investigation into the meaning of proper names is anthropological, not psychological. But such a study has enormous implications for psycholinguistics. Insofar as linguistic anthropology changes our views about what children acquire when they acquire language, it significantly influences our views not only about how children develop language skills but also

23. I am mindful of the dangers of citing literature as if it were "data," but the following passage affords an important insight even though it comes from the author's "mind" (which in this case provides a very useful "sample of one"!).

24. Even in the Balinese practice described in n. 17 above, in which "one's name is what remains to one when all the other socially much more salient cultural labels attached to one's person are removed," it nonetheless remains the case that a person's name is one of that person's most private and closely guarded possessions (Geertz 1973:370).

25. At the beginning of the novel, despite all the conditioning he has been subjected to, D-503 cannot help but *turn* O-90's number into a *name*: "The switchboard clicked. I raised my eyes—O-90, of course! In half a minute she will be here to take me for the walk. Dear O-! She always seems to me to look like her name, O-. She is approximately ten centimetres shorter than the required Maternal Norm. Therefore she appears round all over; the rose-colored O of her lips is open to meet every word of mine."

about what Geertz (1973) calls "the social nature of thought." By removing the Cartesian argument for internal constraints, one is removing the Cartesian assumption that language development *must* be based on certain conceptual foundations or predispositions (given the "architecture" of the mind and the "privacy" of reference). Of course, it remains possible that empirical investigations will confirm the universal presence of certain concepts or biases prior to language acquisition,[26] but recent research on concepts of space provides us with an important example of how linguistic development drives conceptual development (see Bowerman 1996, Brown 2001).

Brown and Levinson have shown that different cultures employ different frames of reference to organize their spatial experience (see Brown and Levinson 2000 for an explanation of the difference between "Absolute," "Relative," and "Intrinsic" systems). What is perhaps most striking about this research is how, contrary to the Piagetian assumption that the development of spatial concepts can be predicted on the basis of a priori reasoning about their logical order, we find support for the Vygotskyan argument that children's development of spatial concepts is shaped by the spatial terms employed in their culture. Thus, to paraphrase what Brown and Levinson (2000) argue, if the semantics of linguistic expression L presupposes a conceptual distinction D, then evidence that child/adult uses L correctly *is a criterion for saying* that the child/adult is employing D.

The crux of the logical argument being presented here is the Wittgensteinian claim that using "to the left of" and "to the right of" correctly means that S *must* have grasped these relative concepts. But whereas the Cartesian treats this statement as describing a mental phenomenon, Wittgenstein focuses on the logical character of the "must" in this proposition. His argument is that "to say that S uses 'ϕ' correctly *means* that S possesses the concept ϕ, or, that S 'sees what is common' to the exemplars of ϕ" (1960:135). That is, the criterion for saying "S sees what is common to all the samples of ϕ" or "S understands what language-speakers are referring to when they speak of 'ϕ'" is correct use of the concept-word "ϕ": the criterion for saying the one is the criterion for saying the others (Shanker 1998). Thus, the ultimate response to the sceptical dilemma on which Quine sought to base his argument for the radical indeterminacy of translation is that we cannot understand what it would mean to suppose that, for example, a child could use "N.N." correctly, in all the appropriate circumstances, but not know whom "N.N." refers to. To say "S uses 'N.N.' correctly in all the appropriate circumstances" *just is to say* "S knows whom 'N.N.' refers to."

There can be no doubt that the stages through which children progress in their gradual mastery of linguistic skills are related to maturational factors (just as is the case with the mastery of other skills). But this hardly means that "language acquisition is a matter of growth and maturation of relatively fixed capacities, under appropriate external conditions. The form of the language that is acquired is largely determined by internal factors" (Chomsky 1966:65). It is not built-in linguistic constraints but, rather, children's strong desire to be socially accepted, the actions of their caregivers and peers, and the communicative dynamics involved in their affective development that lead and enable them to conform to the linguistic behaviour of those around them. Thus, the answer to our opening question about how our views about the nature and development of language must change once our thinking is freed from the dictates of Cartesian epistemology is that the study of language development can no longer be governed by nativist assumptions about the internal constraints that must determine how children acquire language. What linguistic anthropology has shown us is not how children learn to apply their "innate knowledge" of language but, rather, the kinds of things that children in different societies actually learn when they learn how to speak and how they acquire these skills (Ochs and Schieffelin 1995).

26. One could certainly cite evidence to support such an argument (see, for example, Baillargeon 1987 on the infant's early grasp of object permanence).

Comments

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It is abundantly clear that human beings have some sort of genetically based program for acquiring language. Otherwise we would, as a species, be speechless (we would not be able to sign, either), the normal condition among subjects of the animal kingdom. It is also clear that, at least with respect to our more elaborated forms of linguistic communication, our nearest relatives, chimpanzees, do not have this programming. A necessary caveat at this point, however, is that we clearly share some of our linguistic competence with apes and the extent of the sharing is not yet known. Two interesting questions emerge from these observations. What is the nature of the programming—that is, how specific is it? And what are the nature and extent of the genetic differences that make it possible?

With respect to the specificity of the programming, two other fairly obvious observations bear repeating. First, there are many different languages in the world—seemingly an argument against specificity—but they all have certain things in common—apparently an argument in favor of specificity, hence the debate over the existence of a "universal grammar." Second, there are many other aspects of human behavior that are universally acquired and that appear to be under some constraints with respect to their variability. The rest of this we call culture. So, if we have a "language acquisition device," then we must also have a "culture acquisition

device"—or, perhaps more properly, devices. I should state the obvious here—although some linguistic formalists have claimed it, it has never been demonstrated that language is any more complex or any more tightly constrained than numerous other aspects of culturally determined behavior. Furthermore, as Shanker points out, the cultural cannot be disentangled from the linguistic, as competence in a language is ultimately measured by the appropriate use of its terms in social, that is, cultural, settings.

With respect to the way in which all of this is genetically programmed, given our marked genetic similarity to chimpanzees the human increment must be packed into a relatively small segment of the genome. We can, however, attempt to make some predictions about this from what little is actually known about the nature of those small genetic differences. What is most striking about the human lineage is that it introduces a paucity of novel anatomical structures—the differences between chimpanzees and humans appear to involve developmental differences in maturation rates producing proportional changes in limbs, trunk, and head. Most striking, of course, is the vast increase in the size of the brain relative to the rest of the body, producing a much larger brain, particularly with respect to the cerebral cortex, but one that is otherwise not remarkably different from those of chimpanzees. This represents a continuation of a long-term evolutionary trend in the primates toward larger brains and more flexible behavior. Nowhere do we see evidence for the emergence of new "instinctive" responses—the trend is in the opposite direction.

Shanker rightly criticizes the Neo-Cartesian linguists for assuming a theoretical stance that points in directions opposite to those I have just outlined. He points us, I think, in the right direction as we attempt to resolve the many competing arguments that have been made concerning these issues by promoting what I interpret as an interactionist and explicitly anthropological approach to language acquisition. In particular, he makes instructive use of the example of proper names. This example is so telling because proper names are seemingly so straightforward with respect to reference, picking out, as each of them does, just one object. It is in the emergence of meaning and in the complexity of the culturally determined rules for their use that these terms gain interest. The Neo-Cartesian approach, with its simplifying assumptions, denies the richness of these socially emergent connections between a language and the culture within which it functions.

Shanker shows us that the meaning of a proper name, say, David F. Armstrong, is much more than the extension of the term, that is, the male human being sitting in front of a computer on the campus of Gallaudet University composing this comment. I can't be said to know the meaning of my own name until I learn that it is not appropriate to use it in full except in very limited situations, such as when signing this comment before submitting it to the editor. But how do I come to learn the rules governing its use, and how can Shanker know whether I know its meaning? I can only learn these rules through years of more or less appropriate interactions with my fellows, and Shanker can know that I have succeeded only by observing how I behave in a large variety of complex social situations. That these rules are not learned automatically or "instinctively" is shown by the existence of an English word, "malapropism," that refers to our frequent failures to apply them correctly. So the language acquisition device cannot be disengaged from the culture acquisition device—the two are inextricably intertwined—and the unit thus identified already has a name. It is called the brain.

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There have been many criticisms of "nativism" in "Cartesian linguistics" attacking positions that neither Chomsky nor any well-known generative grammarian has ever thought to defend. Shanker's polemic is no exception. It involves two spurious claims: (1) that Cartesian linguistics (hypothesizing universal, grammar-specific principles that structure any language acquired through usual interaction with the learner's community) vitiates an understanding of language structure and use and (2) that nativism permits linguistic anthropology only to "validate" and "apply" (wrongheaded) generative principles. Briefly, Chomsky (2000) outlines a language system of the human brain. The language system reflexively discriminates and categorizes parts of the flux of human experience as "language" and develops complex abilities to infer and interpret this highly structured and structurally peculiar type of human production. There is nothing intrinsically different about the language system—concerning innateness, evolution, or universality—when compared with the visual system, the immune system, the respiratory system, or any other complex biological system. Much polemic is driven by distaste for "innateness," "genes," and "evolution." Historical and ideological reasons—some well-justified—explain this aversion. None bear on universal grammar.

Regarding 1, nothing of substance in Shanker's claim pertains to the truth or falsehood of empirical proposals about specific language structures such as word order (each language has a specified *linear* order linking *arguments to predicates*) or thematic role (roles assigned to noun-phrase positions, such as subject-agent and object-patient in transitive sentences). Without word order and thematic role there is no apparent way of distinguishing "X hits Y" from "Y hits X" or "Hits YX" from "Hits XY" (Baker 1988). Chimpanzees (or dogs) may share many intellectual faculties with humans, including abilities for adopting propostional attitudes (beliefs, desires) or symbolizing referents with arbitrary signs (Shanker's language "Rubicon"). Without syntactic principles, however, they cannot disambiguate from signs alone the different references that different possible or-

ders of propositions may describe. The only example hinting at rudimentary representational order in apes comes from Kanzi, a bonobo (Savage-Rumbaugh, Shanker, and Taylor 1998). Kanzi, though, evinces no consistent subject-predicate structure. His action-action combinations (e.g., "Chase Bite") employ two "predicates" and no subject. No human language allows sentences that have no arguments and so cannot express a proposition. More significant, neither Kanzi nor any other nonhuman creature recursively embeds structured strings within strings (clauses, sentences), allowing almost limitless expression and production of information.

Logically, if a mind can take fragmentary instances of experience (relative to the richness of the whole data set) and spontaneously predict (generalize) the extension of those scattered cases to an indefinitely large class of complexly related cases, then the inferential structure responsible for prediction cannot derive from the experience. Acquiring language structure via "social interaction" is no more plausible than learning by "osmosis." As Hume noted, structure must be prior to experience, just as the cranes and architectural drawings used in building must exist prior to any initial construction. If humans are organisms whose species attributes emerged through the same evolutionary processes governing all other species, there is no alternative to a priori mental structures' being evolved biological structures (as adaptations or by-products of adaptations).

The language system is no more (or less) "autonomous" from the ambient social environment or other mental systems than the visual system is detachable from ambient light and object patterning or other physical systems (including, in humans, linguistic and other cognitive systems of meaning [Marr 1982]). Neither system exists or develops in isolation, both being subsystems of even more intricate structures (Hubel 1988). Cartesian claims of biological "autonomy" for the language system or the visual system refer only to a specifiable level of systemic functioning within a system hierarchy.

Developmental and cognitive psychologists have identified several structures in human cognitive systems relating to the interface between the language system and these other systems (Spelke, Phillips, and Woodward 1995). One is the "whole-object constraint" (Carey 1985). Children of whatever culture or language assume by default that nouns apply to whole objects (a rabbit) and not to object parts (a piece of leg, a patch of fur, disparate patches of fur and leg) or an object-and-its-environment. Such "innate bias" helps resolve indeterminacy in translation. An anthropologist who visits an exotic tribe and sees a tribe member pointing to something that the anthropologist identifies as a running deer is fairly safe in assuming that the tribe member also thought of pointing to a running deer (and not a moving deer part or a shifting pattern of fur-and-grass) even if the tribe member also believes that the deer is someone's ancestor. If this weren't the case—if radical indeterminacy were omnipresent—anthropology would be impossible.

Shanker notes that before learning to talk children engage in joint attention, contingently interact with others to achieve goals, and so forth. This supposedly undermines the Cartesian model. Yet, much work in this area—called the child's "theory of mind" or "folk-psychology"—focuses on nativist issues: How do children reliably *infer* rich mental structures about *other minds* from a few gestures and without mastery of language? How do children infer that people's mental structures (intentions) cause others to act a distance (without physical contact)? The emerging consensus is that children are biologically endowed with a "theory of mind" that matures with predictable cross-cultural regularity over the first three years of life (Avis and Harris 1991, Leslie 1994, Baron-Cohen 1995). Some features of the developing theory of mind interface with maturing features of the developing language system in systematic ways now under experimental study.

Nothing in Shanker's comparison of Navajo and Anglo-American naming relates to, much less undermines, generativist claims. Many generative grammarians accept proper names as "rigid designators" that refer to something outside the mind and whose true meaning may never be known by any mind (e.g., the true reference of "Homer" or "Los Angeles" [Longobardi 1994]). Granted, proper names customarily have wider connotations for Navajos than for Americans, but what has this to do with the language system or English? Navajos who speak English presumably use Navajo proper names in English as in Navajo. English-speakers may use proper names as Navajos allegedly do: to be a "Roosevelt," a "Kennedy," or a "Windsor" also implies social obligation.

Similar considerations apply to Shanker's discussion of Brown and Levinson. Some Australian Aboriginal languages make little use of prepositions that express relative spatial position ("to the left of," "in front of"), relying instead on terms for absolute positioning (cardinal points). Native speakers have trouble learning English prepositions in Australian schools. This may have more to do with difficulties in interfacing with the cognitive system for relative spatial positioning than with language structure as such. Thus, Midwestern farmers more likely use absolute positioning than New York City folk in giving directions or locating places, and New Yorkers probably have a harder time understanding "to the southwest" than "to the left of."

Regarding 2, no evidence supports Shanker's claim. Generativists show that culture-specific choice of grammar can have wide-ranging implications for interfacing with other cognitive systems that affect "worldviews" (in the sense of systematic variations on universal mental structures). Thus, in Warlpiri, an Australian language, syntactic structure permits incorporation of indefinitely many subordinate propositions in a single clause (Hale 1986), and therefore Warlpiri can directly convey causal coincidence and complementarity in time, space, or circumstance (*Nantuwu-ka-parnka-mi-mata* = "horse-run-tired," meaning "the horse is tired while it is running"). Themes of complementarity and coincidence pervade kinship and myth.

Biologists believe that all life consists of universal,

highly structured codings of biological information (DNA, proteins, genes, cells). Still, biologists go on to explore diversity at many different levels (bacteria, species, individuals, phyla). Similarly, linguistic anthropology can use generative grammar to comprehend the diversity of languages and the cultural worlds they describe. This includes the very issues about proper names that Shanker highlights (Louis 1998).

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This is a complex and interesting paper addressing a set of issues basic to anthropology because they are basic to the way human languages work as elements of linguistic practice. Shanker's argument pits nativists against linguistic anthropologists on the question of what it means to learn and know a language. For nativists, knowledge of language is innate and species-specific. It is based on a capacity that is specifically linguistic, fundamentally mental, and universal to humans. For linguistic anthropologists, as Shanker portrays them, knowledge of language is pretty much the opposite: socially mediated and therefore society-specific, derived from many things other than language (e.g., "context"), and highly variable instead of universally invariant. Shanker's linguistic anthropologists are interested in the full range of things that children actually learn as they learn to speak, whereas nativists are interested only in the role of innate knowledge in potentiating the learning process. Linguistic anthropologists are antimentalist and proceed from the historical specificity of ethnography, whereas nativists are exclusively mentalist and proceed from the universal invariance of theory.

This way of framing the debate is potentially misleading, however, in that it equates linguistic anthropology with nonmentalism, noninnateness, nonuniversalism, and the reduction of language to nonlanguage. In contemporary linguistic anthropology these features are actually put together in various ways, and the position for which Shanker argues is but one fairly extreme variant. According to nativism, humans are born with innate, species-specific knowledge of language. When we say "species-specific," we mean universal to humans, essentially invariant across the species. Call this knowledge of language universal grammar, and say that children are born with innate knowledge of universal grammar that potentiates language. It is, then, on the basis of universal grammar that children can come to learn their own language, which will always conform to the constraints and principles of universal language. Furthermore, for nativism what matters most in the learning process is the *mise-en-oeuvre* of universal grammar, without which children could not possibly learn their own language. Languages are so complex and abstract and the "data" accessible to children so limited that the learning process must be based on some other knowledge, something already there at birth. This innate human knowledge of language constitutes a universal base on which language-specific features are superficial variants. Particular languages are then the accidents of history, constrained by the necessity of species. The social is aligned with what is nonuniversal and superficial—not how the species mind processes language but how speakers of a language do such-and-such with talk. This way of posing the question serves nativism well by defining both its own position and the alternatives to it.

Shanker is exactly right to reject this picture of languages and the trivial role it assigns to social life, but I think the critique can be pushed farther. Most of linguistic anthropology lies beyond nativism's bipolar ken. It is both universalizing and committed to close empirical description of actual languages. It treats language as a system for thinking but also as a social construction shaped in relation to the body and actualized in practice. It is mentalist but refuses to reduce language to thought. Perhaps most important, mainstream linguistic anthropology has sought neither to isolate linguistic systems from society nor to collapse language into behavior, as nativism would have it. It is the *coarticulation of form with social context* that linguistic anthropologists study, not the reduction of the one to the other.

What does it mean, then, to treat language as (a family of) social practice(s)? Shanker argues that proper names and, by extension, other forms of singular definite reference are best analyzed from what used to be called a pragmatic perspective. Rather than defining them as formal classes with stable semantic representations, we should treat them as elements of language games. It is the rules of the games and not the rules of grammar that underlie the practice of referring. To echo Austin, referring is one of the things people do with words. But there is a danger in equating practice with Austinian speech acts, language games, or rule-governed routines. As usually conceived, these things are tied to face-to-face interaction, whereas practice cannot be reduced to the face-to-face or the "ordinary." In the work of Bourdieu, Foucault, de Certeau, and other prominent practice theorists, the regularities of practice are never described as rule-governed, and the postulate of the unfettered individual speaker is rejected from the outset. At the same time, there is no necessary rejection of the conceptual realities of speech practices; it's just that mental activities are no less embedded in practice than are other forms of human engagement. A full-blown practice approach to referring must synthesize specifically linguistic features of language with an account of native speakers' ways of interpreting and evaluating language and an equally rigorous and nonreductive account of what actually goes on when agents speak. None of these three factors can be ignored or reduced to the others. Shanker has opened up an important space at the juncture of language and social experience. It will require the joint efforts of anthropologists, linguists, and philosophers to turn that opening into a framework for practice.

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In this interesting paper, Shanker seeks both to lay bare and to repudiate what he considers the Cartesian epistemological assumptions of the nativist view of language acquisition as put forward by Noam Chomsky and, more recently, by constraint theory. At the same time he makes a philosophical plea for an alternative approach to language acquisition known as linguistic anthropology. These two goals are not unrelated; on the contrary, Shanker believes that identifying (and rejecting) the Cartesian assumptions that underpin the nativist view paves the way for the anthropological study of language acquisition. Indeed, given his claim that nativism is a "direct consequence" of Cartesian epistemology, it follows that rejecting the latter undermines the former and thus allows scope for non-nativist views to play a significant role in the study of language.

By "Cartesian epistemology" Shanker means not only the purely philosophical claim that meaning and reference are private mental phenomena but also the view, widespread among psycholinguists, that there are universal properties of the human mind, properties that can only be accounted for on the assumption of nativism. It is unclear to me whether the latter view is Cartesian or can be said to rest on Cartesian assumptions about meaning and reference. The real motive for the search for universal properties of the mind/brain is not so much Cartesian epistemology as what Wittgenstein calls "referentialism": the view that the primary function of many or most words is to refer to things or activities. The point of Wittgenstein's criticism of referentialism in psychology is not that psychological words are not about thoughts and feelings but that the *ways* in which they are about psychological phenomena differ profoundly from the ways in which sentences like "His C-fibres fire" are about C-fibres. Psychological concepts, the later Wittgenstein argues, depend on "patterns of life," and this implies that their use may lack a determinate sense. The standard reaction of the hard sciences to indeterminacy of sense—of which cultural variability is but one example—is to explain it away as an epistemic shortcoming soon to be repaired by the advance of our knowledge of a language-gene. The proper reply to this response is to point out that it rests on a mistaken assimilation of the use of psychological language to physicalist language. In fact, Shanker beautifully shows how the idea of referentialism, rather than Cartesian epistemology, misconstrues the meaning of proper names. His ubiquitous use of the idea of Cartesian epistemology is therefore misleading, even with respect to his own alternative project.

I wonder, furthermore, whether the Chomskyan approach in psycholinguistics and linguistic anthropology are as incompatible as Shanker suggests. Shanker cites Chomsky's "poverty of the stimulus" argument as a historically influential example of Cartesian linguistics, saying that Chomsky concluded that children must (tacitly) know general principles of syntax that could not possibly have been acquired from input but must be built into the mind/brain. Chomsky's critique of B. F. Skinner's *Verbal Behavior*, in the context of which this argument was put forward, is less a priori than Shanker suggests. Indeed, Chomsky concedes that it is beyond question that children acquire a good deal of their (verbal) behaviour by casual observation and imitation of adults and other children, but he emphasizes that it is simply not true that they can learn language only through feedback from the environment (Chomsky 1959:43). There is no incompatibility between this (Cartesian?) view and the non-Cartesian view that considers children as first and foremost developing social agents in their familiar and their larger cultural environment. There is not even an incompatibility between the much stronger view that the mind/brain is genetically pre-programmed for grammatical behaviour—a claim also defended by Chomsky and one that Shanker considers Cartesian—and the anthropological view which attributes a fundamental role to learning. Indeed, the fact that human beings learn is itself genetically determined. I emphasize these points not because Shanker would deny them but rather to counterbalance the "retreat from the brain" that so often characterizes social ("Wittgensteinian") approaches to mind and language. Shanker correctly reminds us that human behaviour (in a very broad sense) provides criteria for what we call "thought," "meaning." and so on, but this in no way entails that the study of the brain can never yield facts relevant for the study of mind and language. As far as concerns the acquisition of language, one can (and should) accept the hypothesis that the brain has the capacity to mediate the acquisition of grammatical behaviour, but this does not amount to the same thing as adopting the (Chomskyan) hypothesis that the brain contains an inherited grammar nerve-net. Wittgenstein himself was keenly aware of the (referentialist?) tendency to project the structure of linguistic rules onto the brain: "The brain looks like a writing, inviting us to read it, and yet it isn't a writing. Suppose humans became more intelligent the more books they owned —suppose that were a fact, but that it didn't matter at all what the books contained" (Wittgenstein 1982: par.806).

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Shanker's article provides much food for thought, both in the challenge it poses to nativist accounts of language competence and in the philosophically rigorous defense it offers linguistic anthropologists for some of their standard working assumptions. Taking as his test case the issue of the presumed universality of personal names, Shanker is able to adduce solid ethnographic evidence

in support of his argument that "to learn the language of one's society is to learn the sorts of things that are done with words" rather than merely to actualize a specifically linguistic competence with which we are innately endowed.

The one way in which I would perhaps take the argument farther is to challenge not only the innateness hypothesis but the assumption that innateness is to "social factors" as the universal is to the culturally or linguistically specific. To see what I mean, let us turn to another form of personal reference that Shanker discusses more briefly, namely, personal pronouns and the corresponding grammatical category of *person* (as signaled, for example, by pronominal affixes on the verb, verb agreement as in English "I am" versus "You are," etc). As Shanker says, pronouns are "indexical." That is, unlike common nouns, whose reference depends on some general sense that can be abstracted from any particular speech situation in which they are used, the value of, for example, the pronoun *I* can only be specified as "the person who is uttering the present instance of discourse containing *I*" (Benveniste 1966:252, my translation)[1] and *you* as "the individual spoken to in the present instance of discourse containing the linguistic instance *you*" (Benveniste 1971:218). In other words, again those of Benveniste (1971:224–25),

> I use *I* only when I am speaking to someone who will be a *you* in my address. It is this condition of dialogue that is constitutive of *person*, for it implies that reciprocally *I* becomes *you* in the address of the one who in turn designates himself as *I*. . . . neither of the terms can be conceived without the other; they are complementary, although according to an "interior/exterior" opposition, and, at the same time, they are reversible. If we seek a parallel to this, we will not find it. The condition of man in language is unique.

By "condition of man in language" I take Benveniste to be referring in part to the fact that personal pronouns are not only without parallel among the communication systems of other species but also, within the human world, a cross-linguistic universal (Benveniste 1971:217; cf. Comrie 1981; Steiner 1975; Brown 1991:133). But they are also a quintessential example of Shanker's point that to learn a language is to learn the sorts of things that are done with words. This applies at a number of different levels of sociocultural specificity, ranging from the very local to the cross-culturally universal. Toward the local end of the continuum we have Shanker's example of the use of first- and second-person singular forms by Polynesian chiefs and New Guinea "big men" to refer to and personify social groups with which they are identified (Rumsey 1999, 2000). At the other end of the continuum there is the common core that *all* uses of personal pronouns have in common everywhere, namely, that *I* refers indexically to the present incumbent of the role of speaker and *you* to that of addressee (however variable the nature of social personae that may inhabit these roles) and that these roles are potentially reversible in the way that Benveniste describes.

The point I want to emphasize is that even at this entirely universal end of the continuum, Shanker's conclusion is still valid: the acquisition of and competence in these aspects of language cannot be adequately understood as a purely organismic phenomenon which is merely "triggered" by "social factors" in the developing child's environment. Rather, a certain basic social scenario—Benveniste's "condition of dialogue" among perspective-swapping human subjects—is built into language itself. And equally, the capacity for perspective-swapping is one for which the human species is highly evolved, in tandem with but partially distinct from its capacity for language (Ricard, Girouard, and Décarie 1999). The forms all of this may take are highly variable, as Shanker shows, but even those aspects of it which are constant need not be taken as evidence for an innate "mental organ" of the kind posited by Chomsky. Language does not subsist solely *within* the human organism. It also lives in the world at large, where it has a long natural history which is intimately bound up with that of the human species both in its species-wide aspects and in its more local, culturally specific ones.

1. "La personne qui énonce la présente instance de discours contenant *je*."

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In his article on the post-Cartesian perspective on names, Shanker has articulated an important "epistemological shift," pointing out that linguistic anthropology has a vital role to play in revising the nativist view on language. He expresses the view that language is not a function of the individual mind/brain and that knowing about naming is not something a brainlike homunculus "cognizes" (as the nativists might have it). Instead, naming and language use in general are activities that presuppose participation in a Wittgensteinian "form of life." Shanker further suggests that probably the most important contribution of linguistic anthropology is not to support or refute nativism but, rather, to discover what the developing child (or, I would add, adult) learns about language through participation in community.

That said, I would propose that the best way to illustrate this important point is go beyond a discussion of how one community manages to use a word like *gavagai* compatibly or even that different communities have different naming practices. Naming in practice can also be clarified by looking at how *identical* names (with the same referent) are treated *differently* across varying communities. A cross-community perspective illustrates, by exemplifying the foundation of meaning in community practices, the *indexicality* of naming and provides a way out of the nativist/behaviorist stalemate. Indexicality, that is, the "subtle ways in which linguistic forms are existentially connected with the situations in which

they are used and the people who use them" (Duranti 2001:32), is critical to a linguistic anthropological understanding of naming practices.

Consider, for example, the nicknames acquired by certain gang members in Los Angeles (Rymes 1996). These names are given on the basis of group membership, and they are earned, usually, through a violent initiation rite. Also, they are often inherited from biological or gang-based kin. For example, someone named Creeper might name a new and promising member Little Creeper. To be a member of a gang means to have a name that indexes that membership and loyalty to a group of friends, or "family." Little Creeper can use his name within this community with pride and respect. However, when he writes this name in a very different community, for example, on a piece of paper at school, he is marked as a dangerous criminal. A principal might even use this name as evidence to have him expelled. Thus, though Little Creeper and the principal share the knowledge that this name is attached to him, they put this knowledge into different kinds of practice according to the community norms with which they are operating. Naming, in this case, is a practice that takes variable forms, according to normative community practices, while the name itself maintains its singular reference: the boy, Little Creeper. Who this boy is considered to be (a loyal friend versus a criminal threat, for example) changes according to the community of practice within which he is using his name.

This example illustrates how an understanding of the indexicality of naming can take our language studies beyond a nativist-behaviorist debate that Shanker has characterized through references to Chomsky and Pinker, on the one hand, and Quine, on the other. Names are indexical of community membership, and the ability to use a name functionally grows out of familiarity with the practices within that community. This example also illustrates the ability of linguistic anthropology to provide a more nuanced description of community than is typically used in discussions of sense and reference. There is not simply one "Navajo" or one "Anglo-American" community into which individuals are socialized. Within these groups, there are multiple communities of practice. Thus, linguistic anthropology supplies the insights not only of cross-linguistic but also of cross-community study.

I want to take this discussion a step farther and claim that these theoretical adjustments have ramifications in public contexts and in the lives of practical people. An understanding of naming as indexical practice (rather than simple labeling) illuminates, for example, the paradox of gang membership—a name can simultaneously index loyalty and criminality. As a result, the consequences for naming can be as severe as expulsion from school or a trip to jail. Linguistic anthropology, then, as a way of understanding naming practices, is capable not only of providing needed relief from a tired Cartesian nativism but also, in a most practical sense, of reconceptualizing the world, the language practices that construct it, and our actions within it.

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Instead of reading Shanker's article as a rejection of nativists' claim that it doesn't make sense to speak of children as *learning* language, I take Shanker to be expounding the important sense in which nativists are right: language is not one of the things that children can be said to learn. The nativist claim is correct, but it is made for the wrong reasons. Let us turn to the reasons I think Shanker provides for questioning whether children "learn" language, reasons having to do with what language is.

Shanker emphasizes the complexity of children's language development: "so much more is involved than just acquiring a repertoire of interactional techniques; more fundamentally, language development involves enculturation into a community's distinctive way of being-in-the-world." Shanker himself avoids the expression "language learning," choosing instead to speak of "language development" and above all of "enculturation," thereby indicating that language is more deeply connected with *how we humans live together* than is the notion of *a* language: *a* language, such as French or German, is usually defined merely in terms of its vocabulary and grammar. Shanker later uses a comparison between Anglo-American and Navajo naming practices to reveal how deeply notions of proper names are rooted in *forms of life*. Names are used in typical human situations and activities, and learning what a name is involves becoming familiar with these situations and activities. When these situations differ, the notion of proper names differs accordingly. A teacher who finds that one of his pupils is missing and asks, "Where is George today?" is not merely asking where the bearer of the name "George" is located. "George," in this situation, is the name of a pupil/classmate whom one can expect to be in school, and the question expresses that expectation. The question "Where is George Bush today?" is different. It contains the name of an official person who is travelling for officially declared purposes. The question, if asked by the same teacher, is aimed at determining whether the pupils are keeping track of where this official person is travelling and for what announced purposes.

It is difficult to imagine what it would mean to understand what a proper name is but not know any of the characteristic situations in which names are used in so many natural ways. These situations could metaphorically be described as the *home* of proper names. We normally don't have to acquire mastery of these aspects of the human life form again when we learn to speak a second language, for we came to master them when we began to speak the first time. Children's language development, then, is essentially connected with becoming human, with becoming a natural participant in our human situations and ways of doing things together; that is where words belong and are at home, where they have their uses and natural points.

I would like to call language, in the sense investigated by Shanker, our *primary language*, for it is what we acquire when we begin to speak the first time. Our primary language is more than our mother tongue, which is merely *a* language: it is our human life with words. I take Shanker to be saying the following: Playing football, riding bicycles, and studying second languages are delimited human practices that we can survey and describe in our primary language. We can point at someone and say, "She is practicing football right now, and she has been doing so for an hour." We can even use a clock to determine exactly when she began and when she stopped practicing football. But we cannot point at a little child and say, "She has been practicing language for an hour, but before that she was just playing," for playing is part of language development, as is everything else children do. We can, however, point at a university student and say, "She is practicing Spanish, and she has been doing so for an hour." In contrast to the second languages we may learn as adults, then, our primary language is not one of the things we can be said to learn.

Shanker is investigating, in my view, the sense in which there is a point in nativists' view of language acquisition as a maturational process. But we must keep in mind that this process is not a speculative hypothesis about the child's brain, and we don't need neurophysiological research to discover it. The maturational process of acquiring our primary language is open to view. Shanker shows that anthropology has a better chance of understanding this process than linguistics, which tends to confuse human language with grammatical representations of the second languages we may learn when we already know how to speak.

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Shanker makes clear early in his article that his aim is to attack not only "the nativist view of language acquisition" but also "the Cartesian assumptions that underpin the nativist view." What these Cartesian assumptions are is something that must be gathered from brief remarks scattered throughout the article. Thus he writes, "Quine embraces from the start the standard Cartesian assumption that *meaning* and *reference* are mental phenomena, which as such are epistemically private." Later on, he suggests that Frege, Kripke, and the various philosophers who have held that a proper name has the same meaning as a cluster of descriptions are all Cartesians and continues, "All Cartesian theories have accepted that referring is a *mental act* as opposed something that *agents* do when they use words to perform speech acts."

There is not much point in arguing about whether these philosophers really did hold the views Shanker attributes to them, as many of the phrases he uses in characterizing their views, especially "epistemically private" and "mental act," are phrases whose meaning is not very clear. Given that such phrases do not have any universally accepted meaning in either everyday or philosophical usage, Shanker would need to explain what they mean for his historical claims to have any clear substance. Still, it is very surprising that any philosopher should be thought to have denied the obvious truth that agents sometimes refer to objects. Far from denying it, it is clear that Frege often affirmed it, at least if the verb "bezeichnen" in his writings may be translated by "refer to." For example, in "On Sense and Reference" he wrote, "The reference (*Bedeutung*) of a proper name is the very object which we refer to (*bezeichnen*) by means of it."

Shanker does not present any textual evidence to show that Quine, Frege, and Kripke actually did hold the "Cartesian" views that he attributes to them. What of the nativists, whose nativism is said to be "underpinned" by Cartesian assumptions? Shanker does not define "nativism," but he does say that the most important contemporary nativist is Chomsky. Presumably he is referring to the doctrine that we all owe our success in learning our native language to having been born with some very rich and detailed set of relevant dispositions. At any rate, I shall use "nativism" to refer to this doctrine. In saying that the latter underpin the former, Shanker presumably means not just that Cartesian assumptions entail nativism, for one could agree with that and also agree that the Cartesian assumptions are absurd without thereby conceding that there is anything wrong with nativism. A false thesis can entail a true one. Shanker presumably wishes to suggest that nativists consciously accept the Cartesian assumptions and use them in defending their nativism. Once again, however, he presents no textual evidence.

If nativism is false, there must be some alternative explanation of the phenomena it was meant to account for. One of the main arguments for it goes something like this: Children growing up will hear a certain limited set of sentences being pronounced by other people, but they will also be able to express themselves with a high degree of success using sentences that they have not heard others using. How does this happen? Chomsky insists that it cannot be explained by supposing that children make "inductive generalizations." He is on firm ground here: given any set of, say, 100 sentences in some language, countless false grammatical hypotheses about that language will be inductively supported by reference to that sample set, just as well as any of the true ones.

Does Shanker have any alternative explanation of the phenomena for which nativism was meant to account? He seems to want to present a brief sketch of one in the very last paragraph of his article. He writes, "It is not built-in linguistic constraints but, rather, children's strong desire to be socially accepted, the actions of their caregivers and peers," etc. This seems to miss the point of Chomsky's problem, which was not simply to explain how children succeed in speaking like other people in their community (that could be achieved by saying as little as possible or by not pronouncing a sentence unless they had first heard someone else using it) but to account for the fact that children succeed in producing gram-

matically correct sentences even when they have not heard those sentences before.

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It may be useful to read Shanker's paper in the light of earlier texts which have expressed similar kinds of objections to the Cartesian view of language. Here I want to draw attention to two such texts.

In *Über die Verschiedenheit des menschlichen Sprachbaues und ihren Einfluss auf die geistige Entwicklung des Menschengeschlechts* (1830), Wilhelm von Humboldt argued that language is not purely the product of some innate categorial thought but emerges from communicative interaction. It is, as Paul Grice would later put it, the result of sustained cooperation between interlocutors. The "I," von Humboldt said, depends on the "You" whose power of thought radiates back to the "I," and vice versa. As the "I" and the "You" engage in discourse, their very use of language presumes the other's power of thought. It presumes that the "You" will try to understand the "I." Thus language is a process, the emerging and creative work of individual minds interpreting each other and reinterpreting their cultural heritage: "As each language has received its substance from unknown earlier periods, the work of the mind which generates the exchange of thoughts is always simultaneously directed towards something given, not purely creating, but refiguring" (Böhler 1995[1830]:38, my translation).[1] This is echoed by Shanker when he sums up his findings by saying, "It is not built-in linguistic constraints but, rather, children's strong desire to be socially accepted, the actions of their caregivers and peers, and the communicative dynamics involved in their affective development that lead and enable them to conform to the linguistic behaviour of those around them."

Of all the modern (and post-modern) anthropological linguists it is perhaps Stephen Tyler who, in *The Said and the Unsaid* (1978), has offered the most thorough critique of the Neo-Cartesian or Chomskyan view of language. With von Humboldt, he starts from the fact that speech is central to the formation of consciousness and the self, that "it is only through others that the self reveals itself and comes to know itself as something more than an object" (p. 141). But then, more boldly than anyone before or after him, he goes on to envisage a rhetorical theory of mind, meaning, and culture. He does this against a historical background which he sketches as follows (p. 167):

> With the decline of rhetoric, meaning was separated from the speech event, and the notion of speech or speaking subordinated to the idea of language. Meaning by the seventeenth century had become almost entirely a property of words rather than deeds, as revealed primarily in the rationalist philosophers' [read Descartes's] equation of thought and language and in their identification of language as the limit of reason. To this development the empiricist philosophers added their interpretation of the distinction between reason and passion. Thought is divided into the rational and the passionate. The voice of reason is literal, the voice of passion poetic or metaphoric. This separation of reason and passion has destroyed the ethical basis of discourse.

Our task is to heal this "Cartesian rift," as David MacDougall (1998) has aptly called it, and overcome the alienated and alienating separation of reason and passion prevailing in the human sciences today. Shanker's critique of the idea that one might study children's acquisition of language solely within free-standing, decontextualized communicational systems, his outline of the social and cultural factors involved in the learning of proper names, and his insistence that "proper names are bound up with each culture's deepest views about the autonomy of the individual, the ties of familial relations, and one's larger social and ethical obligations" are therefore steps in the right direction.

But it seems to me that we could go a bit farther and consider the problematic of proper names and language acquisition within the framework of the rhetorical theory of language foreshadowed by von Humboldt and advocated by Tyler. This would allow us to bring out more clearly that what children know when they know what a name is must necessarily be always incomplete. Rhetoric builds on the insufficiencies of language and exploits them. This is why the rhetorical theory of language keeps a keen eye on the gaps and lapses in communication and alerts us to the fact that behind the said there is always the unsaid, behind the spoken the unspeakable.

Like so many philosophers of language (Austin, Grice, Searle, et al.) and so many "functional" linguists (Levinson and Brown, Sperber and Wilson, Ochs and Schieffelin, et al.), Shanker is close to rhetoric but never consciously connects with it. This causes him to overlook, as I have said, the fact that knowledge of proper names must always be fragmentary.

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Shanker raises interesting issues concerning the practices whereby proper names are used to name individuals in different cultures. I would like to add to his contribution by drawing attention to some of the grammatical features of proper names which enable these practices to occur. In the first place, the distinction between com-

1. "Da jede schon einen Stoff von früheren Geschlechtern aus uns unbekannter Vorzeit empfangen hat, so ist die . . . den Gedankenausdruck hervorbringende geistige Tätigkeit immer zugleich auf etwas schon Gegebenes gerichtet, nicht rein erzeugend, sondern umgestaltend."

mon nouns and proper nouns is not an absolute or all-or-nothing one. The linguistic notion of instantiation is important here (see Halliday and Matthiessen 1999: 14–15). Language users organize and interpret experience in terms of ordered hierarchies of categories which are internal to the organization of a particular language system. These hierarchies of categories exhibit a scale of what Langacker (1987:377–86) has called schematicity—a scale of both semantic generality and semantic specification. Higher-order or more schematic categories have fewer semantic specifications; lower-level ones have more and more specific specifications. A noun is the grammatical class which realizes the semantic category Thing. The category Thing is understood as the most schematic category of the many more specific "things" specified by the lower-level instantiations of the schematic category Thing. Thus, "skyscraper," "bird," "quark," "cell," "prisoner," and so on, are more specific instantiations of the schematic category Thing. Moreover, the semantic specification of "sparrow" is more specific than that for "bird" such that the latter is schematic with respect to its lower-level instantiation. In this perspective, proper nouns may be seen as maximally specific instantiations of the schematic category Thing in contrast with common nouns, say, which are relatively less instantiating.

In the clause "Harry Butler caught a taipan," the proper name "Harry Butler" designates a specific individual who is identifiable by the interactants in some discourse context, whereas "a taipan" designates an arbitrary instance of the category in question. That is, this nominal group is both nonspecific and nonreferential, whereas the proper name "Harry Butler" is both specific and referential. That is, it specifies a given entity as a discourse participant which, once it has been so specified by the instantiating and categorizing functions of the nominal group, can be tracked through discourse by subsequent pronominal and other mentions as referring to the same participant. However, proper nouns, while maximally specifying in terms of the hierarchy described above, do, nonetheless, construe a schematicity relation, though this is attenuated as compared with the degree of schematicity evident in a common noun. The point is that the difference is one of degree; it is not an absolute distinction. For a start, proper nouns specify a unique instance of some (set of) schematic categories. The semantic specification "human: male" is schematic to the name "Harry Butler" at the same time that "Harry Butler" is *not* schematic to "human: male." The instantiation relation is a one-way relation—from the schematic category to the more specific one. This is also demonstrated by the fact that I can say of some unworthy political figure, "He mistakenly thinks he's another Winston Churchill," where "another Winston Churchill" refers not to the historical individual, the wartime prime minister of Great Britain, but to a nonspecific though individual instance of the "Winston Churchill" type to which the "he" in the sentence mistakenly believes himself to conform. This shows that the proper noun "Winston Churchill" has schematic properties such that it can be used to categorize specific instances (Davidse 1992:105). My main point here is that the act of referring cannot be an epistemically private mental act because it depends on the lexicogrammatical and semantic resources of the language used. The semantics of referring depends on those categories of "thing" that are built into the intrinsic organization of a given language system. A given act of referring necessarily requires reference to a grounded instance in some discourse of the nominal meanings that are part of the organization of a given language. Leaving aside here any question concerning the universal set of meanings that may or may not be possible across all languages, I shall say that the more limited though nevertheless vast and highly complex set of categories made possible by the grammatical resources of the noun for categorizing and interpreting the phenomena of human experience as various classes of Thing in a particular language are *cultural* categories which are, however, necessary for the functioning of the individual's brain in ontogenesis.

These proposals can help to clarify that reference is a semiotically mediated process or activity in which, to use the Peircean terminology, the object referred to is represented in the activity of interpretants through the mediation of representamena. In this way, we can avoid the problem posed by the Cartesian approach, whereby reference is an epistemically private mental act, at the same time that we retain the important insight that reference to an object—perceived or imagined—is a semiotically mediated cognitive act embedded in situated human activity. Just how the representamena is construed as referring to its object depends on the interests and orientations of the agents involved in this activity in motivating an interpretant in responding to the representamena as a sign of the object referred to (Whitson 1997:144).

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Shanker's paper deals with questions of fundamental importance. In my view, however, it is flawed insofar as it couches these questions in terms of false alternatives. This applies, above all, to the central question: "Should the account of language acquisition be Cartesian or non-Cartesian?" "Cartesian" means "nativist," and "nativists" fall into two kinds again: "the Chomskyan nativists" and "the constraint theory nativists." Shanker is right, in my view, to reject these two nativisms as totally inadequate, but he makes his task of refuting "nativism" very easy by ignoring an entirely different "nativist" theory, not so much "Cartesian" as "Leibnizian" (cf. Wierzbicka 2001*a*). This theory, which colleagues and I have been developing since the mid-sixties, proposing it from the start as an alternative to Chomsky's so-called Cartesian linguistics, is known as the natural semantic me-

talanguage theory. In fact, Chomskyan linguistics was never fully "Cartesian," because Descartes's interest in language was intrinsically linked with his interest in the content of human thought, that is, with semantics, whereas Chomskyan linguistics was from the start profoundly non-semantic. In particular, Chomskyan linguistics ignored Descartes's key assumption (developed by Leibniz) that there are innate and indefinable human concepts which provide the bedrock of human cognition and communication.

In the course of the past three decades, this fundamental idea was used as a guiding principle in a vast body of empirical cross-linguistic investigations into semantic universals within the natural-semantic-metalanguage framework, resulting in substantive hypotheses about innate and universal human concepts and their combinatory properties. The set of universal human concepts uncovered includes 60 concepts identifiable in terms of specific lexico-grammatical elements in all languages (cf. Goddard and Wierzbicka 1994, n.d.; Wierzbicka 1996, 1997; Goddard 1998, n.d.). These concepts are I, YOU, SOMEONE, SOMETHING/THING, PEOPLE, BODY; THIS, THE SAME, OTHER; ONE, TWO, SOME, ALL, MANY; GOOD, BAD; BIG, SMALL; THINK, KNOW, WANT, FEEL, SEE, HEAR; SAY, WORD, TRUE; DO, HAPPEN, MOVE; THERE-IS, HAVE; LIVE, DIE; WHEN, NOW, BEFORE, AFTER, A-LONG-TIME, A-SHORT-TIME, FOR-SOME-TIME; WHERE, HERE, ABOVE, BELOW, FAR, NEAR, SIDE, INSIDE; NOT, MAYBE, CAN, BECAUSE, IF; VERY, MORE; KIND-OF, PART-OF; LIKE. Shanker says that "recent research on concepts of space provides us with an important example of how linguistic development drives conceptual development," but he doesn't seem to be aware that empirical cross-linguistic research has identified certain conceptual universals of space, including ABOVE, BELOW, and INSIDE, which can be seen as providing a (presumably innate) basis for diverse conceptual and linguistic development in any given language.

In his neo-behaviourist emphasis on "social practices" (to the exclusion of any innate conceptual tools in terms of which experience could be organized by the individual), Shanker fails to draw a distinction between universal (presumably innate) human concepts and those which are culture-specific (and therefore clearly *not* innate). He is oblivious to the fact that studying "social practices" requires a conceptual framework and that without an independently justified conceptual framework one usually ends up using, unwittingly, the framework of one's own native language—in most cases, English. He doesn't ask in what language (i.e., in what conceptual framework) social practices and their acquisition should be discussed, and he ends up by attributing to all cultures such blatantly Anglo concerns and values as "self-esteem" and "autonomy" and using terms like "intimacy" as universally applicable interpretive categories (cf. Wierzbicka 1991, 1992, 1997). Having acknowledged some commonalities in Anglo-American and Navajo use of proper names, Shanker concludes: "What is much more striking that these commonalities, however, is the differences." He doesn't say, however, why we couldn't—or shouldn't—be interested in both differences and commonalities, and he doesn't seem to see that we cannot study the differences between cultures without some common measure and that if this common measure is not to be ethnocentric it must be based on concepts and categories occurring in all cultures (and likely to be innate).

I am very much in sympathy with Shanker's emphasis on the need to study language acquisition in its cultural context and to see the links between the acquisition of language and the acquisition of culture. In my view, however, he is mistaken in seeing "nativism" of any kind as either opposed or irrelevant to the study of cultures in general and of the acculturation of children in particular. As I see it, only a well-conceived "nativism" can provide us with adequate conceptual tools for studying social practices and their acquisition cross-culturally.

Shanker says that "nativism allows no scope for linguistic anthropology to play a positive role in our understanding of how we know what we know about language." For Chomskyan and post-Chomskyan nativism this is true, but a "nativism" linked with an empirical search for conceptual and linguistic universals provides both linguistic anthropology and the study of language acquisition in a cultural context with a necessary conceptual foundation and an effective analytical framework, and it bears fruit in descriptive practice (cf., e.g., Wierzbicka 1999, 2001*b*; Harkins and Wierzbicka n.d.).

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I agree with Shanker that linguistic anthropology presents a broadening of view from abstract linguistic competence to embodied social competence (of which language use is an important part). Another way to strengthen the argument would be to move from the exclusive consideration of naming to the wider field of social deixis (Zeitlyn 1993). This would enable him to consider acquisition of pronouns (which he mentions) and of kin terms (e.g., Carter 1984). Overall, when studying linguistic praxis, linguistic anthropologists take inspiration from pragmatics (Levinson 1983) and from cognitive psychology (Clark 1996). Meaning takes a relatively back-seat position, since the focus is not on isolated (more or less solipsistic) individuals but on groups of people interacting.

Shanker discusses the radical-translation problem, in which Quine presents a *reductio ad absurdum* argument against a denominative theory of meaning. Translation is impossible only if meaning is denominative, that is to say, if the meaning of words or phrases is modelled on the meaning of proper names; change the account of meaning and Quine's radical-translation problem vanishes. However, as Shanker says, if the problem arises in radical translation then it also occurs with our neigh-

bours. So, if (big if) the basis of meaning is as assumed, then we are forced into a form of solipsism. We are not solipsists; ergo, the theory of meaning is wrong. Another way to respond to Quine is to remark that the radical-translation problem is not as normally described because everyone can perform interpretive acts which are tantamount to translation. Keesing (1985) uses the "problematic" of translation to urge caution in the search for "metaphysics." Anthropologists are apt, he suggests, to mistake "conventional metaphor" (which implies no metaphysical commitment) for metaphysical assertion. However, he does not doubt the possibility of translation but simply advocates caution and sensitivity.

Some papers by Robert Feleppa (1986, 1982) discuss this issue as part of the "emic/etic" debate. Feleppa argues that a translation should not be seen as a set of descriptive hypotheses and therefore that it is not susceptible to Quine's underdetermination-by-evidence argument. Instead, he says, translations have more in common with rules, especially in that both are "violable" without being refutable. Therefore he is able to agree with Quine that translations lack truth values, but he maintains that "they still have an empirically legitimate rôle, akin to that of technical definitions and rules of inference" (1986:249). Translation establishes (or codifies) the framework within which facts are expressed. It is thus a necessary and important step in any ethnographic description but is not susceptible to the same sorts of criticisms levelled at "the facts" (pp. 248–49). It is notable that Feleppa cites neither phenomenologists, ethnomethodologists, nor sociolinguists in his bibliography. Scheff (1987:365), in a short reply to Feleppa, quotes Steiner and makes the telling comment "His argument about translatability is empirical in the sense that there is a community of bilinguals to whom we can appeal this (or any other) translation." This leads us to the possibility of "back translation." Feleppa and Quine are both guilty of the "denial of coevalness" (Fabian 1983). The anthropological subject is seen as "Other," and no dialogue is possible. Bilinguals can and do discuss the adequacy of translations and thereby confute the radical-translation problem. Indeed, Quine (1960:47) allows for this possibility but describes it as a "costly" solution: "We can see a way, though costly, in which he can still accomplish radical translation of [non-observational occasion] sentences. He can settle down and learn the language directly as an infant might. Having thus become bilingual, he can translate the non-observational occasion sentences by introspected stimulus synonymy." Dummett (1981:615) calls this the "anthropological solution," and indeed that is exactly what it is. He also says (pp. 376–77): "If there is communication between human beings at all, it must be possible for them to adopt some determinate scheme of intertranslation." In effect, ethnography presents the solution—as Shanker argues in his conclusion.

Reply

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I have tried to look at these replies in the manner of a scientist who has just inserted a catalyst into a social situation and is closely observing the agents' reactions in order to ascertain whether any significant patterns emerge. (Please note that in the psychological as opposed to the chemical use of the term, the catalyst itself is not thought to remain unchanged by this process.) The point of this sort of publication, as I see it, is to see what sorts of questions the target paper raises and then assess whether or in what way these questions might be productive.

It strikes me that the replies reveal a noteworthy pattern on precisely the matter of this "meta-question." Those who are most sympathetic to the generativist framework can see little point in the sort of exercise undertaken here (Atran and Lois, Stirton). Those who find the generativist framework highly constrictive feel that the paper raises important questions that need to be explored in much greater depth (Armstrong, Hanks, Rumsey, Rymes, Segerdahl, Strecker, ter Hark, Wierzbicka, Zeitlyn). Each paper raises important points that demand careful scrutiny, but three closely interrelated issues stand out:

1. What is the nature of Cartesianism? (see Stirton, Strecker, ter Hark)

2. What is wrong, as ter Hark puts it, with saying that "the mind/brain is genetically pre-programmed" for linguistic behaviour? (see Armstrong, Atran and Lois, Wierzbicka)

3. What is the nature of *language*? (see Armstrong, Atran and Lois, Hanks, Rumsey, Rymes, Segerdahl, Zeitlyn) These are very much the questions that I was hoping to raise in this paper.

Ter Hark asks whether the epistemological assumptions that I have described as "Cartesian" are better understood as the result of "referentialist" assumptions about the use of psychological terms. I am not sure that I see these arguments as mutually exclusive, but I certainly think that ter Hark is raising an important question about the sources of Cartesian epistemology. Moreover, his reply serves as a warning not to construe "Cartesianism" too narrowly, for example, as a distinctive doctrine stemming from the writings of Descartes. Rather, one can find elements of "Cartesian" thinking in medieval and even classical Greek writings.

Stirton is also concerned about the notion of "Cartesianism" that I have sought to use, so it might be useful to review briefly some of the key principles that I had in mind. When I spoke about "Cartesianism" I was thinking of such assumptions as that (1) the mind of an isolated individual must impose order on reality by framing mental constructs; (2) subjects' behaviour serves as *evidence* for the mental processes or states they are ex-

periencing or for the concepts they have constructed; (3) one *infers* from their behaviour whether others experience the same mental states or possess the same concepts as oneself; (4) terms such as "meaning," "understanding," and "reference" are the names of mental processes or states; (5) language is some sort of rule-governed system for communicating (encoding and decoding) thoughts from one mind to another; (6) in acquiring language children are somehow "mapping" words onto concepts; (7) we can never know for certain what concept a child has mapped onto a word; and (8) children could not acquire the same "word-concept" mappings as the other members of their community unless their minds were guided by some sort of "built-in constraints."

A point that I was not thinking of at the time but I agree is extremely important is the one Strecker makes about the Cartesian bifurcation between reason and the emotions. As he puts it, the human sciences need to "heal the 'Cartesian rift"' and thus overcome "the alienating separation of reason and passion." This point has especially important implications for the manner in which we study language development. The psycholinguistic community has virtually ignored the role of emotional development in a child's language development, even though anthropologists have shown that there is a strong connection between emotional development and culture (see Litz and White 1986).

What is so interesting about the Cartesian view of emotions is that, like language, they are conceptualized as a sort of "instinct." The "basic" emotions are thought to consist in the activation by an external stimulus of a predetermined sequence of physiological, behavioural, and experiential events (Ekman 1980). The stereotypical facial expressions associated with a "basic" emotion—which apparently are pan-cultural, universal (in the sense that they emerge, at roughly the same age, in blind as well as sighted children), and homologous with nonhuman primate facial expressions—are treated as a sign that the "basic" emotions are just such a sequence of physiological, behavioural, and experiential events. In other words, not only are the facial expressions, movements, postures, etc., associated with a "basic" emotion predetermined and automatic but so is what a subject feels (Griffiths 1997).

Thus, as is the case with language, the Cartesian view of emotions ignores the role of experience in children's development. Emotions are treated as part of their genetic birthright; the "information" for the construction of an "emotion module" is thought to be somehow contained in genes and "potentiated" by appropriate stimuli. What such a modularity approach overlooks, therefore, is how social experiences shape children's emotions. To be sure, certain physiological conditions make it possible for children to enter into the sorts of nurturing relationships that enable them to share and thereby develop certain emotions (Fogel 1993). These emotional experiences in turn affect how their central nervous systems function or develop (Schore 1994, Johnson 1997). But neither factor can be reduced to the other, nor can either be ignored in explaining emotional development (Greenspan 1997).

What makes this issue so relevant to the present discussion is that several of the commentators continue to want to look at language development as an autonomous phenomenon. It is clear from the work that has been done with nonverbal autistic children that emotional and linguistic development are intimately connected, for these children typically begin to speak, spontaneously, as soon as they reach a certain level of emotional development (Greenspan and Wieder 1998). It would thus seem to be the case that, as Vygotsky (1987) suggested, one can explain neither children's linguistic nor their emotional development without considering how the two elements interact with one another. Indeed, Barbara King and I have recently explored the possibility that the same points may apply to the striking results that were seen in the research with the bonobo Kanzi (see Shanker and King n.d.).

Thus, the deeper reason this question of whether emotions constitute a critical element of the development of language skills is relevant concerns the issue of whether or in what sense language is somehow genetically predetermined. As Armstrong and ter Hark point out, much as one may be opposed to nativist thinking, one can hardly deny that language is a species-typical human trait. Furthermore, Wierzbicka raises the important point that her research program has documented more than 60 universal semantic concepts. But then, as Gottlieb (1997) shows in his critique of genetic determinism, *universal* does not entail *innate*. One is not compelled to adopt the genetic determinist position that species-typical traits are *canalized* (i.e., strongly buffered from environmental perturbations by an organism's genes); rather, one can adopt the dynamic-systems-theory view that *species-typical circumstances* are essential for the development of species-typical traits.

According to dynamic systems theory, at each level of a developmental system "the effect of any level of influence is dependent on the rest of the system, making all factors potentially interdependent and mutually constraining" (Gottlieb, Wahlsten, and Lickliter 1998:260). Hence, "the minimum unit for developmental analysis must be the developmental system, comprised of both the organism and the set of physical, biological, and social factors with which it interacts over the course of development." But when we look at the biological factors that are imperative for language development what we are looking for is just that, *biological factors*, and not some vehicle for importing implicit epistemological assumptions (e.g., about the child's *a priori* knowledge of the abstract "structure" of language).

The biological factors that appear to be crucial for language development relate to the child's ability to self-regulate, to take in and respond to the world, to engage in relationships with other people, and to engage in co-regulated communicative activities. Also important here are the "executive functions," among them attention, inhibition, cognitive flexibility, organized searching, planning, and working memory (Pennington et al. 1997). All of these factors—reactivity to stimuli, capacity to process sensations, motor control, and executive func-

tions—are themselves dependent on species-typical circumstances for their development. In other words, close nurturing relationships with caregivers are as much part of the biological as of the social factors that are critical for the development of language skills.

The crux of my response to the second of the three major issues outlined above, therefore, is that, in place of the linear, simple cause-effect models that obtain on the maturational view of language acquisition, we need to look instead for bi-directional, context-sensitive, dynamic-systems-oriented approaches to language development. Rather than looking at language development—or any aspect of a child's development—in terms of a genetically determined "neural module" which needs the appropriate "input" during a "critical period" in order to function properly, we need to look instead at how all of the levels involved in the care of the child interact in the emergence of complex skills and abilities (see Shanker n.d.*a*).

It is not just our views about the manner in which children develop language skills that is affected by this shift from genetic determinist to dynamic-systems thinking, however, but, as several of the commentators point out, our views about the very nature of the skills that they thereby develop. I was fascinated by Rumsey's and Zeitlyn's extension of the argument about proper names to the use of pronouns. One of the most curious of the various traits typically seen in young children with autism is pronoun reversal. For some time now theorists have speculated that this phenomenon is bound up with the children's social deficits, but no one has yet formulated a satisfactory explanation of the social-emotional-linguistic ties operating here. Rumsey's discussion of Benveniste's remarks on "the condition of man in language" suggests that there is indeed a complex, interactive process going on in the social-linguistic-emotional nexus.

As Rumsey puts it, the "'condition of dialogue' among perspective-swapping human subjects is built into language itself." We would be reluctant to describe children's communicative behaviours as "linguistic" if they lacked the capacity to engage in dialogic interactions. Language is not at all like a code which suddenly appears in children at some predetermined age; rather, children develop language skills in the context of co-regulating such primal activities as sharing, requesting, imitating, and playing. They are increasingly motivated to use and develop these potential communicational tools *so that* they may achieve context-dependent interactional goals: goals which themselves develop as a function of children's developing communicational environment and their growing abilities and increasingly differentiated affects.

If one assumes *ab initio* that language is "an autonomous, decontextualizable biplanar code" (Toolan 1996: 3), then one is bound to place great emphasis on issues that are designed to test children's possession of such a "code" (e.g., the so-called creativity problem, the "poverty of the stimulus" argument, or "grammaticality" tests). And if one assumes from the outset that acquisition of this "code" can only be viewed as a maturational phenomenon, then one is bound to find reasons that Kanzi's communicative behaviours or those of a child who, through intensive therapy, has recovered from specific language impairment should not be described in linguistic terms (e.g., because of morphosyntactic deficits, the "dual mechanism" hypothesis, or problems that the subject encounters with a quintessentially Cartesian construct like the "theory of mind" task). The possibility that these arguments are a *product* of Cartesian presuppositions about the nature of language is never considered, for one is committed from the start to the premise that there is a categorial distinction between the "language system" as formally defined and other forms of communication. By challenging this discontinuity view one is arguing not simply that language skills emerge from communicative development but, further, that these language skills cannot be divorced from their communicative function (see Halliday 1975).

I have looked elsewhere at the sources of these familiar generativist critiques of ape language research and the positive therapeutic results obtained with children diagnosed with specific language impairment (see Savage-Rumbaugh, Shanker, and Taylor 1998, Shanker n.d.*a*). I certainly do not wish to minimize the importance of the questions that these critiques raise—or for that matter, the questions that they ignore. Rather than trying to summarize that material here, however, I will reiterate the central reason I argue in this paper that linguistic anthropology poses a fundamental challenge to the nativist view that we all "grow" an identical telementational code that enables us to "encode" and "decode" our thoughts. What linguistic anthropology shows us is that *what* children learn when they learn how to speak and, accordingly, the explanation of *how* children learn how to speak cannot be divorced from the sociocultural environment in which these skills and abilities are constituted and nurtured.

Hanks takes this argument forward in a manner which I think will prove absolutely vital to the fortunes of a non-Cartesian approach to the study of language development. His concern about equating practice with rule-governed routines is, I think, an extremely important point and one that needs to be explored in precisely the manner that he indicates. As does Strecker, he suggests that a non-Cartesian approach to the study of referring (or any speech act) demands the combined fruits of linguistics, philosophy, psychology, and anthropology. We need to look at the specific features of a particular natural language, the reflexive practices of the speakers of that language, and what actually goes on when speakers interact with one another linguistically (see Taylor 1997). But Hanks is not suggesting that language is simply too complex a phenomenon to be adequately investigated by any one discipline alone; rather, the very nature of language is such that it demands such a seamless interdisciplinary approach.

Armstrong and Rymes present further powerful reasons, specifically relating to the construct of proper names, for why, in Armstrong's words, "the Neo-Car-

tesian approach, with its simplifying assumptions, denies the richness of [the] socially emergent connections" between language and culture. Rymes's "cross-community perspective" on the use of the same name (with the same referent) provides a wonderful example of exactly this point by showing how the significance of a name varies according to the community of practice within which that name is used. But, as Rymes points out, the import of such a cross-cultural perspective does not simply lie in the fact that it provides us with "needed relief from a tired Cartesian nativism"; at a deeper level, linguistic anthropology provides us with a means of "reconceptualizing the world, the language practices that construct it, and our actions within it." As Segerdahl explains, "Children's language development . . . is essentially connected with becoming human, with becoming a natural participant in our human situations and ways of doing things together; that is where words belong and are at home, where they have their uses and natural points."

There could be no more fitting note on which to conclude this discussion.

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